# FISCAL YEAR 1984 DEPARTMENT OF ENERGY AUTHORIZATION

## (Fossil Energy and Basic Research)

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## HEARINGS

BEFORE THE

SUBCOMMITTEE ON
ENERGY DEVELOPMENT AND APPLICATIONS

OF THE

COMMITTEE ON
SCIENCE AND TECHNOLOGY
U.S. HOUSE OF REPRESENTATIVES

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# 1984 DEPARTMENT OF ENERGY AUTHORIZATION

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THURSDAY, FEBRUARY 24, 1983

House of Representatives,
Committee on Science and Technology,
Subcommittee on Energy Development and Applications,
*Washington, D.C.*

The subcommittee met, pursuant to call, at 9:30 a.m., in room 2318, Longworth House Office Building, Hon. Don Fuqua (chairman of the subcommittee) presiding.

Mr. Fuqua. The committee will be in order.

This is the first in a series of nine hearings to be held by the Subcommittee on Energy Development and Applications concerning the fiscal year 1984 authorization for the Department of Energy's civilian programs.

Today we consider the basic research programs in the Department of Energy, for which the administration has requested just over $1 billion, an amount about equal to the request for the entire National Science Foundation.

The programs include basic energy sciences, university research support, high energy and nuclear physics, and related activities. They support the long-range, frontier science research that is performed at our national laboratories and universities.

DOE's support for basic research in the physical sciences has grown substantially in recent years—and the fact that DOE now ranks third among all Federal agencies in funding for basic research underscores the important role that these programs play in maintaining a leadership position for U.S. science.

For fiscal year 1984 the administration has proposed a 24-percent increase in basic energy sciences, a 16-percent increase in high-energy physics, and a 17-percent increase in nuclear physics. We applaud the administration's commitment to strengthen these programs with initiatives for new, major research facilities.

However, we want to examine several issues closely to insure that these programs are truly responsive to the needs and desires of the scientific research community.

We wish to learn:

How research priorities are being set,

How DOE's programs coordinate with those in other agencies such as NSF and NASA,

How the needs of the university research community are being met, And how DOE facilities and expertise are being made available to our high-technology industries.

In particular, we want to find out more about the new facilities being proposed and how they will best serve the entire research community, not just the national laboratories at which they are to be located.

Today's witnesses are from the DOE's Office of Energy Research. Next week we shall hear from leading scientists and national laboratory directors.

We are pleased to have as our leadoff witness today Dr. Alvin Trivelpiece, Director of the DOE Office of Energy Research. He is accompanied by Dr. Richard Kropschot and Dr. James Leiss.

We are pleased to have you here, Dr. Trivelpiece. We are pleased to hear from you at this time.

[The opening statement of Mr. Carney follows:]

# OPENING STATEMENT OF THE HONORABLE WILLIAM CARNEY (R-NY)

## EDA SUBCOMMITTEE HEARING ON DOE BASIC RESEARCH PROGRAMS FOR FY 1984

FEBRUARY 24, 1983

THANK YOU, MR. CHAIRMAN, FOR THIS OPPORTUNITY TO EXTEND A WELCOME TO DR. TRIVELPIECE, DIRECTOR OF THE OFFICE OF ENERGY RESEARCH IN THE U.S. DEPARTMENT OF ENERGY.

I LOOK FORWARD TO HIS TESTIMONY ON THE RESEARCH PROGRAMS CONDUCTED BY HIS OFFICE. THESE PROGRAMS INCLUDE HIGH ENERGY AND NUCLEAR PHYSICS, BASIC ENERGY SCIENCES, UNIVERSITY RESEARCH SUPPORT, AND RELATED ACTIVITIES. THEY SUPPORT LONG RANGE, BASIC RESEARCH THAT PROVIDES THE FUNDAMENTAL SCIENTIFIC AND ENGINEERING BASE UPON WHICH THIS COUNTRY'S FUTURE ENERGY OPTIONS WILL DEPEND. FURTHER, SUPPORT OF THIS RESEARCH IS AN INVESTMENT IN THIS COUNTRY'S ECONOMIC WELL-BEING, BOTH IN TERMS OF THE KNOWLEDGE PRODUCED AND THE TRAINING OF UNIVERSITY STUDENTS.

THE ADMINISTRATION HAS PROPOSED SIGNIFICANT INCREASES IN THESE PROGRAMS FOR FISCAL YEAR 1984. THE BASIC ENERGY SCIENCES BUDGET IS INCREASED BY APPROXIMATELY 24% ABOVE THE AMOUNT

APPROPRIATED IN FY 1983; THE NUCLEAR PHYSICS BUDGET IS INCREASED BY 16% AND THE HIGH ENERGY PHYSICS BUDGET IS INCREASED BY 17%. I SUPPORT THE STRENGTHENING OF THESE PROGRAMS, AND AM THEREFORE CONCERNED THAT ONE OF THE POTENTIALLY SIGNIFICANT PROJECTS CONDUCTED BY THE DEPARTMENT AT THE BROOKHAVEN NATIONAL LABORATORY, THE COLLIDING BEAM ACCELERATOR PROJECT, FORMERLY CALLED THE ISABELLE PROJECT, MAY NOT BE ADEQUATELY FUNDED. THE DEPARTMENT IS NOT REQUESTING ANY CONSTRUCTION FUNDS FOR THE COLLIDING BEAM ACCELERATOR FOR FY 1984. HOWEVER, UNDER THIS HIGH ENERGY TECHNOLOGY CATEGORY OF THE HIGH ENERGY PHYSICS PROGRAM, THE DEPARTMENT IS REQUESTING $23 MILLION IN COMPARISON TO $18 MILLION IN FY 1983, TO CONTINUE THE COLLIDING BEAM ACCELERATOR R&D PROGRAM TO DEVELOP ACCELERATOR QUALITY SUPERCONDUCTING MAGNETS. FURTHER, DR. TRIVELPIECE, IN HIS PREPARED TESTIMONY, INDICATES THAT THE DEPARTMENT, IN JULY OF THIS YEAR, WILL REVIEW THE FINDINGS OF THE HIGH ENERGY PHYSICS ADVISORY PANEL WHICH IS STUDYING THE COLLIDING BEAM ACCELERATOR PROJECT, AND WILL REVIEW THEIR RECOMMENDATION AS TO WHETHER CONSTRUCTION SHOULD BEGIN ON A NEW COLLIDING BEAM FACILITY AT THE BROOKHAVEN NATIONAL LABORATORY IN FY 1985. I AM CONCERNED THAT THE DELAY IN CONSTRUCTION AUTHORIZATION OF MORE THAN ONE YEAR AFTER A DECISION IS MADE WILL HAVE A DETRIMENTAL IMPACT UPON THE PROJECT AND THE SCIENTIFIC COMMUNITY. IN THE EVENT OF A FAVORABLE REVIEW BY THE ADVISORY PANEL, IT IS HOPED THAT DOE WILL CONSIDER A REQUEST FOR A FY 84 SUPPLEMENTAL APPROPRIATION TO ASSURE TIMELY PROJECT START.

I LOOK FORWARD TO YOUR TESTIMONY AND YOUR EXPLANATION OF THE DECISION OF THE DEPARTMENT OF ENERGY CONCERNING THE COLLIDING BEAM ACCELERATOR.

## STATEMENTS OF DR. ALVIN W. TRIVELPIECE, DIRECTOR, OFFICE OF ENERGY RESEARCH, DEPARTMENT OF ENERGY, ACCOMPANIED BY DR. RICHARD H. KROPSCHOT, ASSOCIATE DIRECTOR, OFFICE OF BASIC ENERGY SCIENCES/OFFICE OF ENERGY RESEARCH, AND DR. JAMES E. LEISS, ASSOCIATE DIRECTOR, OFFICE OF HIGH ENERGY AND NUCLEAR PHYSICS/OFFICE OF ENERGY RESEARCH

Dr. **Trivelpiece.** Mr. Chairman, good morning. I am pleased to have this opportunity to talk about the fiscal year 1984 budget for some portions of the Office of Energy Research. You have my prepared statement and with your permission I wish to have it submitted for the record.

Mr. **Fuqua.** Yes, we will make the entire statement part of the record and if you wish to summarize, please feel free to do so.

Dr. **Trivelpiece.** Before turning to some introductory remarks, I wanted to point out that science and technology are, in my view, an essential ingredient to a long-term economic strategy for the United States. You heard Secretary Hodel and Dr. Keyworth comment in earlier testimony on the fact that the United States and this administration is strongly committed to a program of basic research. This commitment translates into increased budget requests for some of the items that are in the programs that we are going to discuss here today.

This doesn't mean that there is an increase of a few percent for good science at the margin. Rather, there has been a careful selection of a few items for particular emphasis, and we believe that these are important elements in a strong U.S. science and technology enterprise.

I would like to outline some of these initiatives, but before doing so, I would like to provide a little background. On July 19, 1981, you held hearings with this committee to look into the national laboratories. Almost as a direct result of those hearings, the Department of Energy, through its Energy Research Advisory Board (ERAB), commissioned some reviews by ERAB. The ERAB laboratory panel then prepared a report.

In December of this last year, I testified before this committee and described what the department is doing in terms of reviewing those recommendations, and I tried to point out the steps we were going to take in response to them.

The theme of the ERAB report is, in a sense, that we should try to regard our laboratories more as institutions rather than simply looking at them as facilities in which a collection of work is done; to pay attention to their institutional character. In so doing, we should try to redirect them, perhaps a little bit, into scientifically newer and perhaps more productive areas; that we should seek to have them become more involved with industry, in whatever way that might be appropriate and also that we should try to use them in a way that helps support the U.S. higher educational activities.

These are certainly very good suggestions associated with that theme. I believe they are very consistent with one of the important goals of this committee, namely, to try to use the national laboratories in a way that takes best advantage of them, and helps the United States remain a technological leader.

In doing this, this doesn't mean that the laboratories should simply be used as job shops for industry, to simply do casual work for them, or for the universities either.

The first initiative I want to discuss this morning involves the creation of a national center for advanced materials at the Lawrence Berkeley Laboratory. This is consistent with the theme of ERAB and the other things that we have talked about, that we would like to in effect redirect the Berkeley Laboratory along lines that we believe would be useful to them and the Nation.

We hope that this will serve as a model for other laboratory upgrades. It takes advantage of the fact that Berkeley is a unique setting in that it is a laboratory that is closely allied with a university campus, and there is a strong interchange of students and faculty between these two organizations. Also there is a possibility in this case for industrial involvement.

There are quite a few technical features of this facility, but I am not going to dwell on those today. Dr. Kropschot will talk about some of them in his review. Of course, as you indicated earlier, one of the laboratory directors, Dr. Shirley, who is director of the Lawrence Berkeley Laboratory, will discuss some of these technical details in more depth next week.

I really want to talk about the more fundamental question as to why we are doing some of these things. There is an approximate $80 billion-a-year loss to the United States in essentially net worth, due to erosion and corrosion of various materials.

On the other hand, the United States certainly enjoys an edge in many areas of science, because of the materials development that has taken place in the past. Materials, I think, are vital to a successful U.S. high-technology enterprise, and to the economic well-being, and to our ability to compete in the high tech international arenas. So we hope that this facility will help create new materials information so we can better understand materials that now exist. In addition to that, we will train individuals that are skilled in this science. To that extent, the National Center for Advanced Materials is something that I regard as an essential element in a national strategy.

Before going on to other initiatives, I wanted to touch briefly on another subject. There has been some concern expressed by some members of the scientific community that perhaps we have not followed the traditional approach to causing such a facility to come to public attention and to be proposed for support. I am not quite sure what the traditional approach to this is, because in looking back over other facilities that have come into existence, I really can't find what might be referred to as a traditional theme. Nevertheless I hope that what we do and what you do is that we get on with the job and hope that you support this particular initiative.

However, I am sensitive to the concerns that have been expressed by scientists such as Dr. Phillip Anderson, who has written a letter to you, as you are aware. What I am proposing to do, in being sensitive to these concerns, is to create an ad hoc committee that would report directly to me. It would review the technical and programmatic aspects of this new facility in a way that would involve bringing in individuals from industry, from laboratories and from universities, to look over this program.

Certainly there is adequate time, given the length of time over which this project will be brought into existence, to make any improvements that might grow out of constructive recommendations that would come from this kind of a procedure. I certainly don't have any monopoly on good ideas, and we would like to have the universe of people who might want to use this facility have an opportunity to express themselves in a way that would improve its characteristics.

But I would like to reemphasize that I think it is important to get on with the job, and not to have the beginning of the creation of this facility delayed while we go through this review process. I would be happy at some stage to report back to you on the outcome of any deliberations that occur as a result of this ad hoc committee review.

Among the other initiatives is one for $6 million for university instrumentation. As you are well aware, universities around the country have reviewed the situation with respect to the instrumentation that they need for teaching and research in certain areas. It is well known that there has been an erosion of that capability over the years. Our request is a small part of trying to alleviate that condition. It is a small step in the right direction, but I think it is an important one. It is one I hope that you will support.

In somewhat the same spirit we are proposing to upgrade the accelerators at two academic institutions, one at Yale University and one at the University of Washington. This will help provide state-of-the-art equipment for graduate students engaged in the area of nuclear research. I think it is an important ingredient.

We are also proposing $40 million to begin construction of the Stanford Linear Collider at Stanford University's Linear Accelerator Center. The R&D for that has been carried out in the past. We are now ready to begin construction. Dr. Leiss will touch briefly on some of the technical details of that facility.

Finally, I would like to acknowledge and congratulate the scientists at the CERN facility near Geneva for their outstanding efforts in the discovery of the so-called W particle. This is a very important discovery. It is a little difficult to try to place into perspective just exactly what the W particle is and how important it is and what role it plays and so on, but let me try just briefly.

As you know, matter is made up of atoms, and atoms are in turn made up of protons and neutrons and electrons. The protons are now recognized in turn to be made up of still smaller particles called quarks. If you arrange the conditions so that you cause a proton to be collided at a very high speed with its so-called antimatter component, the antiproton, then what happens is from time to time during these collisions a quark and its antimatter counterpart, the antiquark, will collide and annihilate. In the process of this annihilation they create a particle that has a mass at some 90 times greater than that of the proton itself. This is the W particle.

It itself decays by a variety of methods into a collection of other products. By searching through the debris of literally thousands of these collisions, the scientists in this team at CERN have found the decay products, which give evidence of the fact that the W particle was there and it did decay. This is a remarkable event, and they are to be congratulated for their efforts.

The Stanford Linear Collider will cause electrons and positrons to collide in such a way that among the things it will produce will be a close cousin, the so-called Z-zero particle, which also decays into the other constituent parts. The reason that this is important is that the W particle is a piece in a puzzle that confirms some earlier theoretical predictions. Confirming those theoretical predictions then gives greater confidence that some of the other aspects that have been predicted by that theory will appear and the Z-zero particle is one of those.

The Stanford facility will cause these particles to be created and make it possible to study them in a rather easy manner.

I would now like to turn to Dr. Kropschot who will review some aspects of the basic energy sciences program for a few minutes and then Dr. Leiss will talk about the high energy and nuclear physics programs.

[The prepared statement, plus answers to questions asked of Dr. Trivelpiece, follows:]

Statement of Alvin W. Trivelpiece
Director
Office of Energy Research
Department of Energy
Subcommittee on Energy Development and Applications
of the
House Science and Technology Committee
February 24, 1983

Mr. Chairman and Members of the Subcommittee:

I am pleased to appear before you today to discuss the FY 1984 budget request for several of the research programs supported by the Office of Energy Research. The programs that I will discuss today include High Energy Physics and Nuclear Physics under the General Science and Research appropriation. Included under the Energy Supply R&D appropriations are Basic Energy Sciences, Energy Research Analyses, University Research Support, the new University Research Instrumentation program and the Multiprogram General Purpose Facilities program.

The research programs supported by the Office of Energy Research are investments in our Nation's future. I believe that a vigorous program of scientific research contributes to national security, economic growth and, in no small sense, to human culture and understanding. Our ability to solve our economic and technological problems in this decade and beyond will depend on the scope and depth of our scientific knowledge and on the progress that results from continued support for basic research programs.

Let me turn now to the budget request for our programs. Table I summarizes the Energy Research request for the programs that I will discuss today. Under General Science and Research, the FY 1984 budget request for High

## Table 1

### Office of Energy Research
Summary by Program
($ in millions)

| | FY 1983 Estimate | FY 1984 Request |
|---|---|---|
| General Sciences | | |
| High Energy Physics | $422.2 | $490.0 |
| Nuclear Physics | 130.5 | 153.0 |
| General Sciences Program Direction | 1.4 | 2.2 |
| Total | $554.1 | $645.2 |
| Energy Supply - R&D | | |
| Basic Energy Sciences | $282.8 | $350.2 |
| Energy Research Analyses | 2.8 | 2.9 |
| University Research Support | 6.0 | 5.5 |
| University Research Instrumentation | 0 | 6.0 |
| Multiprogram General Purpose Facilities | 34.0 | 40.6 |
| Advisory & Oversight Program Direction | 2.6 | 3.4 |
| Policy and Management | 1.2 | 0.5 |
| Total | $329.4 | $409.1 |

Energy Physics is $490.0 million. The request for Nuclear Physics is $153.0 million; and we are requesting $2.2 million for General Science Program Direction. Under Energy Supply R&D, our FY 1984 budget request includes $350.2 million for Basic Energy Sciences, $2.9 million for Energy Research Analyses, $5.5 million for University Research Support, $6.0 million for the new University Research Instrumentation program, and $40.6 million for the Multiprogram General Purpose Facilities program. Our request also includes $3.4 million for Advisory and Oversight Program Direction and $0.5 million for Policy and Management.

I would like to discuss each of these programs briefly, beginning with those under the General Science and Research appropriation, and then I would be pleased to respond to questions.

## HIGH ENERGY AND NUCLEAR PHYSICS

The High Energy and Nuclear Physics (HENP) programs supported by the Department fund approximately 90% of the nation's research in these fields, with the remainder supported by the National Science Foundation. These programs, which focus on experimental research using the unique facilities at DOE multiprogram laboratories, single-purpose laboratories and dedicated on-campus facilities at leading universities, have been extremely successful over the last 2-3 decades. U.S. researchers in High Energy and Nuclear Physics have won six Nobel Prizes in the last eight years and sixteen in the last 25 years. University scientists play a crucial role in these programs, conducting more than 75% of the experimental and theoretical research. The knowledge, instruments and applications that have resulted from research in these highly technological basic research areas pervade modern American society. They serve as the basis for much of our national defense strategy, for therapeutic and diagnostic medical applications, for generation of electricity with nuclear processes and for a diverse array of applications in industry and other areas of science.

The High Energy and Nuclear Physics programs are closely related in terms of goals, types of facilities and equipment used to reach those goals and the impact that research results in one program have on the other. Although the two programs do have related goals, they have quite different research emphases. Together, these programs seek a deeper understanding of the nature of matter and energy and the four basic forces of nature that interact with and cause changes in the fundamental constituents of matter. The understanding of these constituents and the physical laws governing their interactions form the basis for all of the physical sciences.

All matter that we see every day is made up of chemical elements, such as carbon, the smallest distinguishable unit of which is the atom. Within each atom there is a central core called the nucleus which contains most of the mass of the atom. The inner constituents of the atomic nucleus are protons and neutrons, which are believed to be made up of subnuclear particles called quarks. Other particles called leptons, which interact through the electromagnetic and the weak nuclear forces, also are held to be basic particles.

The Nuclear Physics program strives to attain a better understanding of the structure and properties of these atomic nuclei and the forces that govern their behavior and to determine how the quark substructure of protons and neutrons influences the make-up of nuclei (i.e., how do quarks act when they are imbedded in nuclei?). High Energy Physics research seeks to understand the nature and relationships among the four fundamental forces of nature and to understand the ultimate structure of matter in terms of the properties and interactions of its basic constituents.

Although the focus of each of the programs is somewhat different, most experiments in either the High Energy or the Nuclear Physics program depend on large particle accelerators to produce intense beams of energetic particles. These are required to probe the properties of atomic nuclei, in the case of Nuclear Physics, or to study elementary particles such as quarks and leptons and their interactions in the case of High Energy Physics. The primary experimental technique used is to study the results of collisions of high energy particles with fixed targets or with other particle beams. In these collisions, many entirely new forms of matter can be created by means of a series of energy-to-matter transformations. The study of these dynamic phenomena then reveals the properties of the underlying building blocks and basic forces involved. In general, as the particles studied become more elementary, their component parts are found to be bound together more tightly. Thus, larger, more powerful and complex accelerators, detectors and supplementary equipment are required to carry out the research.

Although progress in High Energy and Nuclear Physics is dependent on the availability of these forefront accelerator and colliding beam facilities as well as on the sophisticated detectors that record the effects of particle collisions, these advanced facilities are only one facet of the program. Progress in each of the fields of High Energy and Nuclear Physics features a continuing interplay of advancements in experiment, theory and technology. HENP experiments are done at the very forefront of theoretical and technical knowledge; and the range of experiments possible is determined by available technology. Physicists frequently have to invent unique equipment and technology to carry out the experiments of greatest importance.

In the attempt to summarize the essence of the HENP programs, it is all too easy to get lost in unfamiliar esoteric terminology. In fact, I have mentioned only a few of the hundreds of already identified or anticipated particles that inhabit the subatomic world. We have not only identified a growing number of particles, but have seen how they relate to one another and how they can be transformed during collisions with other particles in accelerator experiments. We have observed matter change into energy and vice versa; particles change into other particles which then disappear or change into still other particles. The long-standing goal of high energy physicists is to understand these diverse and complex phenomena by formulating a theory which would encompass all of the interactions of matter and energy revealed by their research. These "unified" and "grand unified" theories, as physicists call them, developed over the last 10 years, imply that the diversity existing in the universe today may have evolved from a single force, or process, that existed only when the universe was formed some 20 billion years ago.

These theories aim at linking together, or unifying, the four known fundamental forces of nature. The electromagnetic force describes how atoms and molecules are formed and is the corner-stone to understanding chemistry, electronic communications and biological processes. The weak nuclear force describes processes such as the radioactive decay of nuclei. The strong nuclear force helps describe how the nuclei of atoms can be so small and stable. The gravitational force, perhaps the most familiar of the four forces of nature, explains such things as why rocks fall and planets orbit.

Recent research results have led to an increasingly better understanding of these forces of nature. That understanding is defined in theories that indicate that the weak nuclear and the electromagnetic forces are actually different forms of a single force--referred to as the electroweak force. Two U.S. physicists shared in the 1979 Noble Prize for work on this unification. The 1980 Nobel Prize in Physics was awarded to two U.S. physicists for work that may lead to unification of the strong nuclear and electroweak forces into what is called the grand unification theory. Eventually, it is postulated that the gravitational force will be incorporated into a super unification, thus bringing all of the basic forces into a unified framework.

In this coming year special emphasis in the High Energy Physics experimental program will be focused on testing the predictions of the currently accepted grand unified theories. Experiments will focus on further understanding the detailed properties and implications of the W particles predicted by the electroweak unified force and recently observed in Europe at CERN by a team that included U.S. physicists. CERN is the acronym for the European Organization for Nuclear Research. Experiments will also focus on effects, such as proton decay, predicted by theories that attempt to unify the strong and electroweak forces. If experiments currently underway confirm the validity of these theories, there would be a major scientific breakthrough in understanding the basic forces of nature. These new concepts may, in time, lead to an understanding of how the universe evolved from the hypothesized "big bang" to its present state and such interesting questions as whether the universe will continue to expand.

The history of progress in High Energy and Nuclear Physics research allows us to say confidently that we are moving steadily toward our long-term goal of understanding the physical world around us. But we get even more from this research. There have been many spin-offs, including the technology of superconducting materials and devices, radio frequency power generation, materials analysis and medical applications, including methods of treating cancer.

More recently, the use of synchrotron light from particle accelerators opened up totally new research capabilities in chemistry, solid state physics, biology and industrial applications. Free-electron lasers, driven by particle accelerators, promise new capabilities for enhanced laser power and control. Radiation processing of materials, which was born from this science, has many exciting new applications ranging from the manufacture of heat-shrinkable electrical insulation to the sterilization of wastes. Although we cannot predict them in advance, history has shown that these kinds of technological advancements are virtually guaranteed tangible benefits that we expect to continue to reap as a result of our investment in basic research, including High Energy and Nuclear Physics research.

## HIGH ENERGY PHYSICS

Let me turn now to the FY 1984 budget request for High Energy Physics. The FY 1984 budget request of $490.0 million for High Energy Physics compares to the FY 1983 estimate of $422.2 million (Table 2). This increase of $67.8 million would permit a High Energy Physics program with an estimated utilization rate of 55% for the existing accelerators, solid support for university based experimental user groups and theoretical research efforts, continued progress on the Fermilab facility development program, and the initiation of construction on a rapid schedule of the high priority Stanford Linear Collider (SLC) project at the Stanford Linear Accelerator Center (SLAC). The Operating Expenses request for FY 1984 is $349.5 million; the Capital Equipment request is $51.5 million; and the Construction request is $89.0 million.

The high energy particle accelerator and colliding beam facilities that would be operated under the requested FY 1984 budget include the 30 billion electron volt (GeV) proton Alternating Gradient Synchrotron at Brookhaven National Laboratory (BNL), the 500-1000 GeV proton synchrotron at the Fermi National Accelerator Laboratory (Fermilab) and the 32 GeV linear electron accelerator at SLAC. SLAC also has two electron-positron colliding beam facilities for which the linear accelerator acts as the injector. These facilities are called SPEAR and the Positron-Electron Project (PEP). Each of the three national accelerator centers (Fermi, SLAC, BNL) provides a unique set of capabilities that complement the others. Together, they represent the experimental facilities essential for a comprehensive national research

## Table 2

### High Energy Physics
### Budget Authority
### ($ in Millions)

| | FY 1983 Estimate | FY 1984 Request |
|---|---|---|
| Operating Expenses | | |
| Physics Research | $ 92.2 | $ 101.5 |
| Facility Operations | 152.1 | 166.0 |
| High Energy Technology | 76.0 | 82.0 |
| | 320.3 | 349.5 |
| Capital Equipment | 46.4 | 51.5 |
| Construction | 55.5 | 89.0 |
| Total High Energy Physics | $ 422.2 | $ 490.0 |

program in High Energy Physics. The major part of the High Energy Physics funding is required to meet the direct needs of these accelerator laboratories. While these three accelerators provide the experimental data sources for almost all accelerator-based High Energy Physics research in the U.S., about 75% of that research is carried out by teams of researchers from the Nation's colleges and universities. Thus, about three-quarters of the funding provided to the accelerator laboratories can be considered as indirect support of the university program.

The FY 1984 budget request for Operating Expenses of $349.5 million would provide for the first full year of operation of the Energy Saver superconducting synchrotron at Fermilab. The Energy Saver, the first stage of a multi-step development program, will reduce electric power needs and extend operation of the accelerator to 500 GeV for fixed-target experiments. With completion of early phases of the Tevatron II project, the third stage of the development program, operation at energies approaching 1000 GeV should be possible. Priority also would be given to good operation and research use of PEP, the 15 GeV x 15 GeV electron-positron colliding beam facility at SLAC. The FY 1984 Operating Expenses request would provide strong continuing support for university research groups, non-accelerator experiments and theoretical research efforts.

This FY 1984 request also includes funding for Brookhaven National Laboratory (BNL) to continue to develop accelerator-quality superconducting magnets, to develop and demonstrate techniques for fabricating the magnets reliably, and to consider lower cost magnet options. Funding is also provided to develop alternative facility designs that would use the tunnel now in existence at BNL. We anticipate a proposal from BNL at the end of March describing their new facility plans, cost and schedule estimates. DOE plans an in-depth review of the BNL proposal in April; a study of High Energy Physics facility needs will be carried out by a Subpanel of the High Energy Physics Advisory Panel (HEPAP) in June and HEPAP will review their report in July. DOE expects to make a decision in July on whether to initiate construction of a new colliding beam facility at BNL in FY 1985.

The Capital Equipment request of $51.5 million gives priority to Fermilab for equipment related to the major detector for colliding beam experiments at Tevatron I and for secondary beam line components and detectors for 1000 GeV experiments at Tevatron II, and to SLAC for work on a detector for SLC research. To accommodate these needs, other programs remain at a somewhat reduced or near constant level of effort.

The FY 1984 Construction request is for $89.0 million, an increase of $33.5 million over FY 1983 (Table 3). A major portion of this request is to initiate construction on the SLAC Linear Collider (SLC) on a rapid schedule. The goal of this project is to develop and demonstrate the practicality of using linear accelerators with precisely controlled, high intensity and highly focused beam pulses to provide high luminosity electron-positron or electron-electron collisions at energies far beyond those that will be affordable for circular colliders. Luminosity is a measure of the rate at which events can be recorded for a given process.

## Table 3

High Energy Physics
Budget Authority
($ in Millions)

| Construction | Prior Years | FY 1984 Request | TEC | Completion Date |
|---|---|---|---|---|
| Tevatron I (81-E-218) | $ 34.1 | $ 20.0 | $ 82.5 | FY 1986 |
| Tevatron II (82-E-206) | 24.8a/ | 13.0 | 49.0 | FY 1985 |
| Stanford Linear Collider (84-ER-133) | 2.0b/ | 40.0 | 112.0 | FY 1986 |
| Accelerator Improvement Projects (84-ER-131) | N/A | 9.0 | 9.0 | FY 1985 |
| General Plant Projects (84-ER-132) | N/A | 7.0 | 7.0 | FY 1986 |
| Total Construction |  | $ 89.0 |  |  |

a/ Includes $800,00 for PE&D
b/ Represents $1,957,000 for PE&D

Completion of the SLC on a three year construction schedule will give the U.S. earliest access to copious production of a previously unidentified particle called the $Z^0$ (pronounced zee-zero) which is a critical prediction of the electroweak force theory. Also included in the Construction request is the continuation of the Tevatron I and II projects at Fermilab. These projects comprise the second and third stages of the multi-step development program. Tevatron I permits acceleration up to 1000 GeV and will provide a facility for proton-antiproton collisions at 1000 GeV. Tevatron II will further expand the accelerator's capabilities by providing upgrading of experimental areas and beam lines external to the main ring for fixed target experiments. The remainder of the construction request is for Accelerator Improvement Projects essential to maintain the reliability, efficiency and scientific effectiveness of the existing accelerator facilities and for General Plant Projects essential to the efficient and reliable operation of the laboratory sites.

## NUCLEAR PHYSICS

The FY 1984 budget request for Nuclear Physics is \$153.0 million, an increase of \$22.5 million over the FY 1983 estimate (Table 4). Of the \$153.0 million request, \$128.0 million is for Operating Expenses, \$11.3 million is for Capital Equipment and \$13.7 million is for Construction. This budget request provides for experimental and theoretical studies aimed at improving our fundamental understanding of atomic nuclei.

## Table 4

### Nuclear Physics
Budget Authority
($ in Millions)

| | FY 1983 Estimate | FY 1984 Request |
|---|---|---|
| **Operating Expenses** | | |
| Medium Energy Nuclear Physics | $ 52.3 | $ 59.0 |
| Heavy Ion Nuclear Physics | 39.7 | 47.0 |
| Low Energy Nuclear Physics | 12.2 | 13.0 |
| Nuclear Theory | 8.2 | 9.0 |
| | 112.4 | 128.0 |
| Capital Equipment | 10.4 | 11.3 |
| Construction | 7.7 | 13.7 |
| Total Nuclear Physics | $ 130.5 | $ 153.0 |

Because nuclear interactions and the behavior of nuclei and nuclear matter determine the physical properties of our universe, quantitative knowledge of nuclei is essential to any basic understanding of the world about us. This Nuclear Physics program is an important element in America's long term investment in its technological future with nuclear processes. As an ancillary benefit, pursuit of Nuclear Physics program goals provides a national base of knowledge, techniques, instrumentation, and skilled manpower in critical technologies.

In addition to talented scientists, the success of the Nuclear Physics program depends upon the existence and effective operation of large and complex accelerator facilities. These facilities are the essential tools of the nuclear scientist. They provide the variety of particle beams which are required for forefront nuclear research. In FY 1984, our highest priority will be to maintain the capability of these facilities to meet evolving research requirements and to operate these facilities a sufficient number of hours in FY 1984 to meet the needs of the research program.

In FY 1984, emphasis in the Nuclear Physics program will be given to exploitation of new research capabilities provided by successful completion of construction projects in FY 1982 and FY 1983. These capabilities include: (1) the ability to accelerate ions as heavy as uranium to low energies in the SuperHILAC and to high energies in the Bevalac (Lawrence Berkeley Laboratory); (2) the completion of the Bates Electron Accelerator recirculator project at the Massachusetts Institute of Technology (MIT) that provides electron beams for precision electron-nuclear scattering studies at higher energies than previously available; and (3) newly available heavy ion beams at the Holifield Heavy Ion Research Facility (Oak Ridge National Laboratory) and the Argonne Tandem/Linac (Argonne National Laboratory) that will allow high precision heavy ion physics studies at higher energies than previously available.

The underlying quark structure of protons and neutrons must play an important role in the makeup of nuclei. However, understanding and quantifying this role remains an important task before us. High priority experiments in FY 1984 are planned with LAMPF's (Clinton P. Anderson Meson Physics Facility at Los Alamos National Laboratory) intense pi meson beams and with Bevalac's new high energy uranium beam capability. Another important thrust in this quark substructure effort is high energy electron scattering experiments at the Stanford Linear Accelerator Center (SLAC). The FY 1984 Nuclear Physics Accelerator Improvement and Modification construction project request includes $1.65 million for a new injector at SLAC for these experiments as well as for research to understand the magnetic structure of nuclei.

Experiments which examine the weak interaction and properties of the neutrino will also be a major thrust of the FY 1984 Nuclear Physics program. The weak interaction governs beta decay of nuclei and is one of the three fundamental forces which determine the characteristics of nuclei. In FY 1984, highlights of this area of emphasis include research with polarized electron beams at Bates and with LAMPF's uniquely pure and intense muon and neutrino beams.

In FY 1984, emphasis will be given to heavy ion research with ions more massive than calcium and in the projectile energy range above 10 MeV (million electron volts) per nucleon. In particular, the newly available beams at the Holifield, the Bevalac low energy beam line, and the Argonne Tandem/Linac will be used to study the mechanisms by which colliding nuclei exchange mass, energy, and angular momentum. Above 30 MeV per nucleon, the projectiles are moving at velocities comparable with and exceeding the average internal velocity of neutrons and protons inside the individual nuclei. Collisions of target nuclei with projectile nuclei at these energies cause formation of intense concentrations of energy and mass. From these experiments, we expect to learn how nuclear matter acts as it is compressed and how energy moves through nuclear matter.

Of the $128.0 million Operating Expenses request for FY 1984, $66.3 million is needed for operation of Nuclear Physics accelerator facilities, with twelve million of this for electric power costs. This funding will allow use of facilities at the same levels as achieved in FY 1982. The remainder of the Operating Expenses request is for experimental research ($52.7 million) and theoretical research ($9.0 million).

In FY 1984, $11.3 million is requested for Capital Equipment. Of that, $10.5 million is needed to acquire experimental and computational equipment used by research scientists as well as equipment which permits effective operation of the accelerators themselves. The most important type of capital equipment needed is the detection system. New and more sophisticated detectors and analytical systems are needed to isolate the particular nuclear events that can provide new information about nuclear phenomena. The remaining $0.8 million is needed to meet Lawrence Berkeley Laboratory's laboratory-wide requirement for general purpose equipment.

The FY 1984 request for Construction funds for Nuclear Physics is $13.7 million (Table 5). The Accelerator Improvement Project includes $1.65 million for the new SLAC injector which I discussed earlier. This injector would produce electron beams with an intensity 100 times greater than the currents now available. The injector will permit experiments that will improve our understanding of electromagnetic properties of nuclei and of the quark structure of nuclei.

Because accelerator facilities have continuous needs during their operating lives for small modifications or additions that improve their performance and enhance their usefulnesses, the FY 1984 construction request also includes Accelerator Improvement Projects and General Plant Projects funds for such activities at the Bevalac, the Clinton P. Anderson Meson Physics Facility, and the William H. Bates Electron Linear Accelerator. The General Plant Projects request also includes funds for the laboratory-wide needs of Lawrence Berkeley Laboratory.

Work by university-based nuclear scientists is a prominent component of the Nuclear Physics program. University scientists conduct experiments at the national accelerator facilities and at dedicated on-campus accelerator

Table 5

Nuclear Physics
Budget Authority
($ in Millions)

| Construction | FY 1984 Request | TEC | Completion Date |
|---|---|---|---|
| Univ. Accelerator Upgrade (84-ER-110) | $ 8.0 | $ 19.0 | FY 1987 |
| Accelerator Improvement Projects (84-ER-121) | 3.5 | 3.5 | FY 1986 |
| General Plant Projects (84-ER-122) | 2.2 | 2.2 | FY 1986 |
| Total Construction | $ 13.7 | | |

facilities. To maintain the scientific strength and attractiveness of university-based nuclear physics facilities to students, these facilities must be maintained at scientifically competitive levels. In the FY 1984 request for Construction funds, $8.0 million is planned for upgrade of on-campus accelerator facilities at Yale University (New Haven, Connecticut) and the University of Washington (Seattle, Washington). The upgraded facilities will allow continuation of the excellent records of scientific productivity and academic preeminence of these outstanding institutions.

## GENERAL SCIENCE PROGRAM DIRECTION

The FY 1984 budget request for General Science Program Direction is $2.2 million. These funds are required to provide for the salaries, benefits, travel and other expenses associated with 35 full-time equivalents in the Office of the Associate Director for High Energy and Nuclear Physics and associated support staff required to administer these programs.

I would now like to discuss the programs under the Energy Supply R&D appropriation, beginning with Basic Energy Sciences.

## BASIC ENERGY SCIENCES

Although the U.S. is in much better shape today than it was 10 years ago with regard to our reliance upon foreign energy sources, there is a continuing need to expand our energy supply and conservation technology base. This is the primary role of the Basic Energy Sciences program. While the current energy situation has reduced the Department's primary focus on short-term problems, the Basic Energy Sciences role - aimed at the longer term - continues to be very important to the future of the nation. Furthermore, while the pursuit of research by Basic Energy Sciences to broaden the technology base for identified technologies continues to be important, even more important is the need for basic scientific research unconstrained by preconceived notions of which technologies will be important in the future. The Basic Energy Sciences program supports approximately 1200 research projects and provides advanced facilities that are used by university and industrial researchers as well as national laboratory scientists. Research is carried out in material sciences; chemical sciences; nuclear sciences; engineering, mathematical and geosciences; and biological energy research. An advanced energy projects effort that crosscuts all disciplines is also included.

It is difficult to convey the impact of the BES program by citing yearly research accomplishments. Scientific findings form a base of knowledge from which improvements to existing technologies or new ones evolve. Often it is only in retrospect that the true importance of research results can be assessed. However, the following examples of research results in FY 1982-1983 that have enhanced energy alternatives or improved our understanding of energy-related phenomena are illustrative of the benefits to be gained from research in the basic energy sciences.

Research in Materials Sciences has resulted in new information that:

- should improve our understanding of the useful properties of superconductivity, which may lead to advancements such as improved superconducting electrical cables important in several advanced technologies;
- will provide a better understanding of the control of stress corrosion cracking of nickel base alloys, a well-known cause of costly unscheduled outages in nuclear power plants; and
- explains the radiation swelling mechanism of a ceramic material potentially useful as a long-term storage host for nuclear waste.

Research in the Chemical Sciences subprogram has lead to:

- An improved ability to measure the ratio of sulfur-32 isotope to sulfur-34 in the air. This new capability is based on negative ion mass spectrometry which allows much more sensitive measurements than the traditional positive ion mass spectometry. This improvement is critical to establishing a reliable measurement technique that could be used to distinguish among the different kinds of sources of sulfur present in the air. Conceivably the discovery could be used in future efforts to reduce the environmental effects of coal combustion.

- Observance of a phenomenon, called "hot carrier effects", that had been theoretically predicted for semiconductor/liquid interfaces. Hot carriers are electrons or positive charges carrying excess energy that are produced by light in semiconductors. If it can be harnessed, this phenomenon could lead to a doubling in the efficiency of photoelectrochemical solar devices.

Several accomplishments under the Nuclear Sciences program relate to nuclear energy. These include:

- The fluorescence emitted from electronically excited plutonium hexafluoride ($PuF_6$) due to laser exposure of vapor molecules of the $PuF_6$, was detected for the first time. Large differences were found between $PuF_6$ and uranium hexafluoride. These differences will mean that process conditions (e.g., laser wavelengths, temperatures) will have to be different for separating plutonium isotopes and uranium isotopes.

- It was also found in studies of the binding of actinides to naturally occuring organic materials that migration of plutonium through soils rich in these materials would be very slow.

Work under the Engineering, Mathematical and Geosciences subprogram:

- An applied math effort involving automated reasoning provided a clever solution to the problem of showing that single component failures could not inhibit the shutdown of a nuclear reactor. This type of verification is a useful tool in nuclear plant licensing procedures.
- Included a geosciences effort that showed that Columbia River basalts have chemical properties that can provide a safety factor by the immobilizing long lived actinides such as neptunium.

A recent accomplishment under the Advanced Energy Projects subprogram has been demonstration of the scientific feasibility of producing storage and primary batteries with electrodes made of light polyacetylene sheets rather than lead. The successful demonstration of this concept could pave the way for a whole new technology and considerably improve the outlook for electrically powered vehicles.

The Biological Energy Research subprogram has made progress in determining how plant systems sense temperature. It has been found that plants adapt to the environment. Control of plants' adaptive responses could lead ultimately to artifically adapting plants to specific environments, for example, for producing biomass in geographic areas where it is not now economically feasible.

At the heart of this program is the need for program continuity concurrent with reasonable opportunities for growth in newer, emerging areas of science. To continue its effectiveness, the Basic Energy sciences program must: 1) maintain a strong core program -- this involves equipping, supporting and encouraging the scientists involved in our current program activities, 2) have growth opportunity to exploit new, emerging areas with great potential importance to energy and 3) continue to support existing, unique facilities while at the same time providing for needed new facilities and their operation.

Research facilities are an integral part of the overall BES program and I would like to briefly explain the progress we are making in this area. BES has recently completed construction of three important user facilities: the National Synchrotron Light Source (NSLS), at Brookhaven National Laboratory; the Intense Pulsed Neutron Source (IPNS), at Argonne National Laboratory; and the 1.5 MeV electron microscope at Lawrence Berkeley Laboratory (LBL). FY 1983 will be the first full year of operation of these newer facilities. The NSLS will provide intense beams of photons in the ultraviolet and x-ray regime to enable studies of surface properties and structure not possible before. The microscope will permit examination of materials while in a corrosive environment and the IPNS can be used to investigate the fundamental vibrational modes of atoms in solids. In FY 1983 the Atomic Resolution Microscope at Lawrence Berkeley Laboratory will begin operation and the National Science Foundation transfers to BES the responsibility for the operation of the

Stanford Synchrotron Radiation Laboratory (SSRL), our second major research facility for synchrotron radiation. In addition, FY 1982 was the first full year of operation of the Combustion Research Facility (CRF) at Sandia National Laboratory, Livermore. This facility provides advanced and unique laser diagnostic tools to a user community consisting of scientists and engineers from academia, industry and government laboratories, all studying fundamental aspects of combustion. Among the areas investigated during the past year were aspects of soot formation, and nitrogen oxide reduction in engine exhausts. Much remains unknown in the complex and very rapid phenomena of combustion, and the advent of the CRF puts the U.S. in the scientific lead in this field.

Having briefly discussed the goals and accomplishments for the BES program I would now like to turn my attention to the program and budget which we are requesting in FY 1984 (Tables 6 & 7). The total request for FY 1984 is $350.2 million and is composed of $295.8 million for Operating Expenses, $24.3 million for Capital Equipment and $30.1 million for Construction. The increase requested is $67.4 million above the FY 1983 level of $282.8 million and I would like to explain this increase. First, as I have mentioned earlier, we must continue to support a strong core research program including the National laboratories, universities, and industrial communities. Secondly, we must expand and fully utilize equipment and facilities to, at a minimum, keep pace with international competition and

## Table 6

### BASIC ENERGY SCIENCES
Budget Authority
($ in millions)

| | FY 1983 Estimate | FY 1984 Request |
|---|---|---|
| Operating Expenses | | |
| Nuclear Science | $ 30.6 | $ 37.9 |
| Material Science | 108.7 | 125.1 |
| Chemical Science | 70.0 | 75.5 |
| Engineering, Mathematical and Geosciences | 31.1 | 33.7 |
| Advanced Energy Projects | 8.3 | 9.1 |
| Biological Energy Research | 9.5 | 10.6 |
| Program Direction | 3.1 | 3.9 |
| | 261.3 | 295.8 |
| Capital Equipment | 18.1 | 24.3 |
| Construction | 3.4 | 30.1 |
| Total Basic Energy Sciences | $ 282.8 | $ 350.2 |

Table 7

BASIC ENERGY SCIENCES
Budget Authority
($ in millions)

| | Prior Years | FY 1984 Request | TEC | Completion Date |
|---|---|---|---|---|
| NSLS Beam Lines and Support Area Construction, BNL | 0 | $ 2.5 | $19.7 | FY 1986 |
| National Center for Advanced Materials, LBL | 0 | 25.9 | 138.9 | FY 1989 |
| Accelerator Improvement Project | N/A | 1.3 | 1.3 | FY 1985 |
| General Plant Projects | N/A | 0.4 | 0.4 | FY 1985 |
| Total | | $30.1 | | |

our own needs. Finally, we must exploit new research opportunities that appear to hold promise of significant breakthroughs important to energy. The budget that we are requesting for FY 1984 will accomplish these goals.

The principal components of the increases sought above for FY 1983 are:

- First, the FY 1984 budget request provides for a cost of living allowance of $15.4 million and allows a modest $8.9 million growth in the various subprograms in the most promising new research areas. Included are the funds needed to meet increased costs for facility operations, and for equipment replacements and additions, as well as for program direction.

- Second, the FY 1984 budget provides for an Advanced Materials Research initiative of $38.0 million which includes two major construction projects and directly related research and equipment. The new construction projects are the National Center for Advanced Materials at LBL and an expansion of capability at the National Synchrotron Light Source at Brookhaven National Laboratory.

In addition, the FY 1984 budget request includes an increment of $5.0 million for Heavy Ion Fusion Research, a program transferred to Basic Energy Sciences from the Office of Inertial Fusion, Defense Programs.

The first item which I mentioned is fairly straightforward but I would like to spend some time discussing the materials initiative and the transfer to BES of the Heavy Ion Fusion program. The Advanced Materials Research initiative builds on the base of knowledge established by the Materials Sciences program. It will complement ongoing core program activities. It will also help maintain U.S. leadership in the field of materials science. Research in materials sciences contributes to developing new or substitute materials, tailoring materials for specific applications, predicting materials problems and service life, and improving our ability to deal with unforseen materials problems in advanced energy systems. Additional research is required to answer a myriad of materials-related questions and is recommended in a series of studies performed by leaders in the scientific community. This initiative takes a substantial step in meeting the existing and projected needs for new work in surface science and catalysis, materials preparation and characterization, and the concepts behind materials as they are used in devices, both large and small.

Of the $38.0 million request for the materials initiative, $7.7 million is for Operating Expenses, $1.9 million is for Capital Equipment and $28.4 million is for Construction. Included in the construction request would be a National Center for Advanced Materials (NCAM) composed of four new facilities and improvements at the SSRL ($25.9 million). The new facilities would include an Advanced Light Source (ALS) and Surface Science and Catalysis Laboratory (SSCL), an Advanced Materials Synthesis Laboratory (AMSL) and an Advanced Device Concepts Laboratory. Also included in the materials initiative is an expanded capability at the National Synchrotron Light Source ($2.5 million).

The Advanced Materials Research initiative will be organized to include participation by U.S. industry as well as universities and laboratories. Industry will participate in the design and use of the facilities to assure that the research addresses appropriate high-technology needs. For example, at the National Center for Advanced Materials (NCAM), an advisory board composed of scientific leaders from industry, universities, and laboratories will advise the LBL director on NCAM scientific program directions. The NSLS and SSRL already have active industrial participants and advisory groups that effectively interact with the scientific community at large. These new facilities and the involvement of industry will provide otherwise unavailable research capabilities that will improve on the U.S. competitive position in high-technology over the long term.

In FY 1984, the Heavy Ion Fusion Research program will be transferred from the Office of Inertial Fusion, Defense Programs to the Office of Energy Research. Within ER, it will be located under the BES Nuclear Sciences subprogram. The program has been moved in response to Congressional guidance contained in Conference Report 97-342 on the Authorization Act for DOE national security programs for 1982 that heavy ion fusion research, which is directed toward development of new energy sources, should not be funded from the defense inertial confinement program. The goal of the Heavy Ion Research program is to develop heavy ion accelerator technology and perform appropriate ion beam experiments to develop an adequate data base for evaluation of heavy ion inertial fusion drivers and their possible use in civilian power applications of inertial confinement fusion.

I will now describe the Energy Research Analyses, University Research Support, University Research Instrumentation and, the Multiprogram General Purpose Facilities programs. The FY 1984 budget request for these programs is shown in Table 8 following my discussion of the programs. Before I discuss these outlay programs, however, I would like to comment on the advisory responsibilities of the Director of Energy Research.

## ADVISORY AND ASSESSMENT ACTIVITIES

In addition to his outlay program responsibilities, the Director of Energy Research carries out statutory advisory responsibilities in several areas. These include serving as the principal science advisor to the Secretary on energy R&D programs, oversight of the non-weapons multiprogram laboratories and the education and training of scientists and engineers. The Office of Energy Research also provides staff support to the Energy Research Advisory Board (ERAB) whose members advise the Secretary on the Department's energy R&D programs. The Board's membership includes representatives from industry, universities, energy users, state and local governments and public interest groups. ERAB reports directly to the Secretary of the Department of Energy with staff support provided through ER's Director of Science and Technology Affairs staff.

Within the Office of Energy Research, the Office of Field Operations Management (FOM) and the Office of Program Analysis (OPA) support the Director's advisory responsibilities. The Office of Field Operations Management supports the Director with regard to oversight of the laboratories and energy-related manpower development and training activities. This Office has developed several management tools used by the Department in administering its laboratories and R&D facilities across the country. It also continues to manage the University Research Support and the Multiprogram General Purpose Facilities programs, as well as the new University Research Instrumentation program, all of which I will discuss shortly. The Office of Program Analysis, on the other hand, provides energy research analysis support.

## ENERGY RESEARCH ANALYSES

The FY 1984 budget request for Energy Research Analyses is $2.9 million. This program provides the Department with the capability for independent, objective analyses of its technical needs and opportunities. The functions of this activity include assessing the adequacy of the long-range (technology base) research that supports the Department's R&D programs; carrying out independent analyses and technology assessments of the agency's R&D programs; providing coordination and direction in interagency activities such as acid rain research; coordinating resolution of major technical issues and research policy developments; and providing support to agency advisory groups such as the Energy Research Advisory Board. The program is carried out by the Office of Energy Research through studies, workshops, panels and other mechanisms which allow the Department to make use of the best available scientific and technical talent from industry, academia and the National Laboratories.

There were a number of accomplishments in each of the program's functional areas during FY 1982-83. Recent assessments of the Department's technology base included a review by the Fossil Energy Research Working Group, an industry, academic and Government work group who defined R&D needs in shale oil, tar sands and heavy oil. Several program analyses were completed including an assessment of the Office of Energy Research's Basic Energy Sciences program, a review of the research objective of the Battery Storage Program that led to a shift in program emphasis and an assessment that clarified the potential benefit of ethanol as a transportation fuel for the Office of Alcohol Fuels. Recent coordination responsibilities for the Department were related to participation in the Interagency Task Force on Acid Precipitation, the Interagency Working Group on Strategic Minerals Policy and the Interagency Program Board on Civil Operational Land Remote Sensing from Space.

The FY 1984 budget request would provide for up to three technology assessments, depending on their complexity, and up to four program assessments. Research analyses would include assessment of the quantifiable accomplishments of research programs, such as the Office of Health and Environmental Research, and reviews of research that crosscuts agency programs (e.g., acid rain). Coordination activities related to acid rain and civil land remote sensing would be continued and support would be provided to the Director, Office of Energy Research on reassessments of the Department's R&D priorities, programs and technology base. Independent peer review of technical or research subjects requested by principal Secretarial Offices or the Director, Office of Energy Research would be supported (e.g., the Coal Combustion Research Working Group, an industry, academic and Government group, will define research needs in coal combustion by early 1984). Finally, advisory groups, such as the Energy Research Advisory Board, will continue to be supported.

## UNIVERSITY RESEARCH SUPPORT

The FY 1984 budget request for the University Research Support (URS) program is $5.5 million. The goal of this program is to assist in maintaining strong, energy-related scientific and technical research capabilities at universities and the national laboratories. The URS program provides a budgetary and programmatic focus for specialized research and manpower development efforts aimed at securing top quality energy researchers and broadening the pool of experienced scientists from which Government, universities and industry draw. This is accomplished by:

- o Using the unique resources of the Department's National Laboratories to assist in university faculty and student research and training;

- o Maintaining and strengthening university based energy research and manpower development efforts in selected high priority research fields;

- o Increasing the number of students pursuing professional level degrees in energy-related science or engineering;

- o Enhancing the quality of the scientific/technical workforce in the Department's national laboratories and;

- o Helping to expedite the transfer of university-based energy research to potential users in the private sector.

The URS program consists of two major subprograms: The University/National Laboratory Cooperative Research program and the Energy Manpower Development program. The first of these programs, University/Laboratory Cooperative Research has two elements, one on student/faculty training and the other on university reactor fuel assistance. The FY 1984 budget request for these activities is $5 million. Of that $5 million, $3.5 million is requested to support efforts that bring university faculty and students to the national laboratories and other government contractor facilities for research and training. In addition to training for the participants, these activities provide the labs with additional research capability. Support is provided to 10 multiprogram laboratories and related university consortia who, in turn interact with 50 Departmental facilities. During 1983, it is expected that over 1900 faculty and student participants will be involved in URS-supported activities at 32 laboratories and contractor facilities.

The University Reactor Fuel Assistance activity, with an FY 1984 budget request of $1.5 million, provides funds to assist in the purchase of nuclear fuel and related support for university-based nuclear research and training reactors. These reactors are essential to the training of future nuclear scientists and engineers. Fuel for the reactors is fabricated under contract to the Department and then "loaned" to the participating universities for use. Partial support is also provided for the operational expenses of a number of the university reactors when they are shared with researchers at neighboring universities. FY 1984 will be the first year of full production by a new contractor for the fuel required by the higher powered research reactors. Up to six such reactors would be refueled during FY 1984 and 12 reactor sharing grants would be made on a competitive basis. Support would also be provided for costs associated with the transport of spent fuel to designated fuel recovery sites.

The second URS program, Energy Manpower Development, consists of two manpower-oriented efforts. The FY 1984 budget request is $0.5 million. The Manpower Assessment activity analyses the supply and demand of manpower for both current and future energy R&D programs. The Energy Education and Training effort supports activities aimed at encouraging secondary school students to pursue energy-related science and engineering programs at the college level. The FY 1984 budget request would provide for continuation or renewal of prior year budget activities.

## UNIVERSITY RESEARCH INSTRUMENTATION

The University Research Instrumentation program is a new initiative proposed for FY 1984 as part of the combined public/ private sector effort to help alleviate the serious, national shortage of state-of-the-art, scientific instrumentation in the major research universities. The budget request is $6 million. The goal of the new initiative is to help universities strengthen their capabilities to conduct advanced, long-range research in specific energy R&D areas.

Direct access to state-of-the-art instrumentation often determines whether scientists can produce frontier-level results. However, the cost of the equipment is now so high that replacing obsolete equipment or obtaining new equipment is beyond the means of many research-oriented universities. The instrumentation required to study many energy-related processes and materials includes a variety of scanning microscopes, spectrometers, X-ray microscopes and diffraction equipment, laser diagnostic equipment, etc., each of which costs in excess of $100,000. A recent study by the Association of American Universities indicated that "the equipment used in the top-ranked universities. . . has a median age twice that of the instrumentation available to leading industrial research laboratories," with a large fraction of the equipment being obsolete.

During the last few years, the Department's national laboratories have been encouraged to increase the amount of time their research equipment is available to university researchers. Several unique, user-oriented research facilities (e.g., the Combustion Diagnostics Center and the National Synchrotron Light Source) have also been established and are equally available to university, industry and laboratory researchers. Emphasis will continue to be placed on the cooperative use of existing equipment and facilities. However, a significant level of university research involves a large number of researchers (both faculty and students) who require continued access to certain instruments. In such instances the equipment must be available on-campus. Although the Department provides a small amount of support for research instrumentation on-campus, such instruments generally cost on the order of $35,000 or less and do not have the capabilities of the sophisticated instruments referred to above.

The University Instrumentation initiative would focus on university research groups with experience in designated research areas (e.g., combustion, biological energy conversion, catalysis, radioactive waste management, engineering research, etc.) of primary concern to both basic and technology-oriented energy research programs. Those groups supported would be assisted in obtaining the additional sophisticated instruments, each generally costing in excess of $100,000, which would enable them more effectively to conduct advanced research in the designated program areas. Awards would be made following a national competition based on such criteria as the merit and accomplishments of the institution's current applicable energy research programs, the potential for expanding and/or improving these programs if the new instruments where available on-campus, the institution's experience in planning for and using other major research equipment, the availability of similar instrumentation at nearby institutions, the institution's commitment to energy research and its plans for meeting and sharing the total costs associated with the instrument(s).

The $6 million requested in FY 1984 would enable some 20 to 30 university research groups to obtain some of the special research equipment they require to improve their capabilities to conduct energy research. A special interagency committee reporting to the President's Science Advisor, and chaired by the Director of the National Science Foundation, will provide overall policy guidance and ensure effective coordination of this and other agency research instrumentation efforts.

## MULTIPROGRAM GENERAL PURPOSE FACILITIES

The FY 1984 budget request for Multiprogram General Purpose Facilities (MGPF) is $40.6 million (Table 9). The program's goal is to rehabilitate and replace multiprogram general support facilities that are essential to continued operation of the laboratories. These facilities include roads, railroads, site utilities and support buildings such as laboratories, office buildings and warehouses. The MGPF program is managed by the Office of Energy Research for the Department.

The major multiprogram laboratories and other Department of Energy sites included in the program received over $3 billion in FY 1982 to carry out research programs in nuclear energy, fusion, basic energy sciences, high energy and nuclear physics, life sciences, solar energy, fossil energy and conservation. During stringent budget years it is often tempting to postpone necessary replacement or rehabilitation of deficient facilities in favor of maintaining a maximum amount of program effort in these research areas. We have learned from the experience of our predecessor agencies that such expediencies lead to negative impacts on the very programs they are aimed to protect. Equally important, they also result in an inventory of billions of dollars in corrective measures which some future generation will be expected to undertake. We plan, therefore, to continue the MGPF program, which began in FY 1981, as a necessary cost of doing business as long as the multiprogram laboratory facilities are utilized to perform research and development functions for the agency.

The FY 1984 budget request would continue construction of nine projects started in prior years and would initiate four critical projects: rehabilitation of Old Town area utilities, LBL (TEC $4.8 million); Cooling Water Facility, ORNL (TEC $4.0 million); road repairs (various sites - TEC $16.0 million) and a Facilities Support project (various sites - TEC $4.0 million). The Facilities Support Project provides for much needed facility-related improvements at several sites that are in the $1-2 million range. Projects in this size range are too large for traditional General Plant Projects funding and are too small and numerous to qualify for line separate item status.

## ADVISORY AND OVERSIGHT PROGRAM DIRECTION AND POLICY AND MANAGEMENT

The FY 1984 budget request for Advisory and Oversight Program Direction is $3.4 million. These funds are required to provide for the salaries, benefits, travel and other expenses associated with 44 full-time equivalents. This FY 1984 request would provide staffing resources required by the Director of Energy Research to carry out his responsibilities under legislation (P.L. 95-91) and those mandated by the Secretary in areas beyond the scope of ongoing Energy Research programs.

The FY 1984 budget request for Policy and Management is $0.5 million. These funds are required to provide for the salaries and related expenses associated with 8 full-time equivalents in the Office of the Director of Energy Research and the Office of Management.

Mr. Chairman, this concludes my testimony. I would be pleased to answer any questions at this time.

## Table 8

### Energy Research Analyses, University Research Support,
### University Research Instrumentation, Multiprogram General Purpose Facilities
Budget Authority
($ in millions)

| | FY 1983 Estimate | FY 1984 Request |
|---|---|---|
| Energy Research Analyses | $ 2.8 | $ 2.9 |
| University Research Support | 6.0 | 5.5 |
| University Research Instrumentation | - | 6.0 |
| Multiprogram General Purpose Facilities | 34.0 | 40.6 |

## Table 9

### Multiprogram General Purpose Facilities
Budget Authority
($ in millions)

| | Prior Years | FY 1984 Request |
|---|---|---|
| **Construction** | | |
| 84-ER-101 Rehabilitate Old Town Utilities, LBL | 0 | 1.0 |
| 84-ER-102 Cooling Water Facility, ORNL | 0 | 0.6 |
| 84-ER-103 Road Repairs, various sites | 0 | 3.0 |
| 84-ER-104 Facilities Support, various sites | 0 | 4.0 |
| Prior Year ongoing projects | 34.0 | 32.0 |
| Total | 34.0 | $40.6 |

COMMITTEE ON SCIENCE AND TECHNOLOGY
U.S. HOUSE OF REPRESENTATIVES
WASHINGTON, D.C. 20515

February 17, 1983

Dr. Alvin W. Trivelpiece
Director, Office of Energy Research
Department of Energy
Washington, D.C. 20585

Dear Dr. Trivelpiece:

In connection with your scheduled appearance at a hearing on February 24, 1983 before the Subcommittee on Energy Development and Applications, I should like you to address the attached list of questions, which concern the FY 1984 DOE budget request for basic research programs.

Your responses to these questions could form part of the testimony or could be submitted separately to become part of your hearing record.

We would appreciate your responses to these questions no later than March 6, 1983, as they will aid the Subcommittee in its actions on the DOE budget request.

Sincerely,

DON FUQUA
Chairman

DF:Hwm

Enclosure

COMMITTEE ON SCIENCE AND TECHNOLOGY
U.S. HOUSE OF REPRESENTATIVES

FY 1984 DOE Authorization Hearings
Questions for the Director of the Office of Energy Research

## BASIC RESEARCH

### General Issues — Basic Research, National Laboratories, Universities

1. The Department's own Energy Research Advisory Board (ERAB) has recommended that DOE's policy toward its national laboratories be revised to give the labs greater latitude in dealing with industry.

   What are you doint to implement these recommendations?

2. DOE is requesting substantial increases for its basic research programs. In part, these increases are required for the construction and operation of large, complex research facilities.

   How are you setting priorities for facility needs in each of your basic research activities?

3. With basic research facilities becoming increasingly more complex and expensive, what are the opportunities for international collaboration in the construction and operation of the next generation of facilities, particularly in high energy physics?

   What steps are you taking to foster international collaboration and cost-sharing?

4. The university research community has expressed dissatisfaction with its limited participation in DOE research activities.

   What are you doing to address the concerns of the university research community?

   What is DOE's proper responsibility for improving university scientific equipment?

5. We hear from university representatives that the changes NSF is making in its grants to give grantees clear responsibility to manage research will enhance the effectiveness of the management of research. Would you consider taking the approach NSF has taken?

6. Many university researchers have programs of research that are artifically fragmented by the multiple support of several federal agencies. This interferes with the effective management of research. NSF is trying to deal with this problem by permitting grantees to make determinations that projects are scientifically or technically related and thereafter regard them as a single project for research management purposes. Would you consider joining NSF in doing this, so long as your Program Officers could approve relatedness in each case?

7. There are complaints in the university community that your use of contracts instead of grants for the support of research interferes with the management of research by causing unnecessary delays and proliferation of paper. Why do you use contracts, which are for procuring services for federal use, for the support of university research?

8. The Office of Energy Research supports work in computational science in the fusion, high energy physics, nuclear physics and basic energy science programs.

   Please provide the Committee with the amount of funding in FY 1983 and FY 1984 for computational science from each of these programs.

   How is this research coordinated within the Department of Energy and with the research being funded by the National Science Foundation and DOD?

9. In the past few years there has been a series of budget structure changes involving the General Science and Research appropriation account. Why were these changes necessary?

   Does the FY 1984 budget structure represent a stable arrangement, at least for the next few fiscal years?

## Basic Energy Sciences

1. The Basic Energy Sciences programs in materials sciences, chemical sciences and nuclear sciences support several large facilities at the national laboratories. The operating expenses for each of these programs can be divided into funds which support research at the national laboratory facilities and funds which support research elsewhere.

   For each of these programs, please provide a table that indicates the operating expenses for each major facility in the program and the operating expenses for all other activities in the program for FY 1983 and FY 1984.

   In the category of other research in these programs, please indicate the amount going directly to universities.

2. In basic energy sciences, how do you determine which subprograms have management responsibilities for which major facilities?

   Specifically, the Stanford Synchrotron Radiation Laboratory is in the nuclear sciences subprogram budget, whereas the National Sychrotron Light Source is managed by the materials sciences program, with operating funding fom both materials sciences and chemical sciences. How do you rationalize this arrangement?

3. The budget request indicates that the operating expenses associated with the National Center for Advanced Materials includes $4.5 million for a forefront research program.

   Will all these funds be expended at the Lawrence Berkeley Laboratory in FY 1984?

   What funds will be available to assist other users to prepare themselves for the NCAM facilities?

4. Please compare the proposed NCAM sychrotron radiation facilities at LBL and SSRL with the proposed upgrade to NSLS -- with respect to energy, brilliance, and other relevant physical properties.

   What evidence does the Department have that substantiates the need for a significant new syrchrotron radiation center?

5. The continuing operating expenses for the current program at the Stanford Synchrotron Radiation Laboratory (SSRL) are included in the Nuclear Sciences program. In FY 1984 an SSRL upgrade is also included as part of the NCAM project.

   Please provide the total operating and construction expense budget to be provided to SSRL in FY 1984.

   Now that the transfer of SSRL to DOE is complete, please provide the Committee with any plans to increase the SSRL support staff. How is this reflected in the 1984 budget, as compared to FY 1983?

6. With respect to the Combustion Research Facility at Sandia Livermore, please provide a table which indicates total support for the facility available in FY 1982, FY 1983 and FY 1984.

   Please indicate the funds available to CRF from the Basic Energy Science program, the Fossil Energy program, and the Conservation program.

7. The heavy ion fusion activity will be conducted by Lawrence Berkeley Laboratory and Los Alamos National Laboratory; please provide the funds available to each in FY 1984.

8. At DOE's request, the National Academy of Sciences has conducted special studies on the needs in applied mathematical sciences.

   Please provide those studies to the Committee.

## Nuclear Physics

1. In FY 1984, $153 million is requested for nuclear physics.

   By facility, please provide the operating funds required for each of the national laboratory facilities and the four university laboratories in the program.

   Please distinguish between expenses to maintain and operate the facility and expenses for associated research and development.

   Also, please provide a similar breakout for FY 1983.

2. The FY 1984 budget contains $8 million for upgrades of the accelerators at the University of Washington and Yale University; the total cost of these upgrades is $19 million.

   Please provide the operations requirements for each facility in 1984 and succeeding years.

3. What is the long range DOE commitment for constructing, maintaining and operating university based facilities for nuclear physics research?

4. Does the FY 1984 Nuclear Physics Construction request of $13.7 million represent the planned level of annual construction for this program over the next several years?

   What is the appropriate percentage of total nuclear physics funding that should be invested in construction of new facilities?

5. What are the highest priority construction items for future years (FY 1985-87)?

6. Do you anticipate any significant change in the scope of the DOE nuclear physics program (for example, the balance between the "medium energy" and "heavy ion" activities)?

7. In FY 1984 the Bevelac biomedical activity will be funded directly in the heavy ion nuclear physics budget. Will future biomedical research at nuclear physics facilities be supported directly by the nuclear physics program?

## High Energy Physics

1. What is the significance to the U.S. high energy physics program of the reported discovery of the W particles at CERN?

2. To what extent are the current U.S. and CERN facility construction programs complementary or duplicative?

3. Is there parity between U.S. researchers using foreign HEP facilities and foreign research teams using U.S. HEP facilities?

4. Please provide the Committee with FY 1984 and outyear DOE funding for Dr. Teng's detector at CERN/LEP.

5. Please provide the Committee with FY 1984 and outyear foreign funding for detectors and other equipment at U.S. facilities.

6. What are the major milestones during FY 1983 and FY 1984 leading to a decision on the Colliding Beam Accelerator facility at Brookhaven?

7. In FY 1984 how much HEP funding would support non-accelerator experiments?

   Do you anticipate this component of the DOE program to grow as a percentage of the total program?

8. By what process are you planning for the next generation HEP facilities (beyond the Tevatron complex, SLC and CBA)?

**Department of Energy**
**Washington, D.C. 20585**

March 10, 1983

Honorable Don Fuqua
Chairman, Subcommittee on Energy
Development and Applications
Committee on Science and Technology
House of Representatives
Washington, D.C. 20515

Dear Mr. Chairman:

Enclosed are answers to the questions you submitted to Dr. Alvin W. Trivelpiece with your letter dated February 17, 1983. These questions and answers are in connection with the February 24, 1983, hearing before the Subcommittee on Energy Development and Applications.

If you have any questions, please call Ingrid Nelson or Tom Pretorius of my staff on 252-4277. They will be happy to assist you.

Sincerely,

Thomas H. Pretorius
for Robert G. Rabben
Assistant General Counsel
for Legislation

Enclosure

## BASIC RESEARCH

1. The Department's own Energy Research Advisory Board (ERAB) has recommended that DOE's policy toward its national laboratories be revised to give the labs greater latitude in dealing with industry.

What are you doing to implement these recommendations?

The Energy Research Advisory Board recommended that: (1) we allow laboratories to do more reimbursable work for others by relaxing constraints, (2) we permit joint ventures with industry, and (3) we allow laboratories to respond to Requests for Proposals from industry cooperative groups. We are already taking actions within current policies to increase the flexibility of the laboratories in dealing with industry by removing barriers. For example, we have recently issued a class waiver of patent rights for fully reimbursed work for others. I think we may see greater use of the laboratories by the private sector because of the class waiver.

In light of the ERAB recommendations, we are reviewing our current policies to determine whether they should be changed to give the laboratories even further latitude in dealing with industry. One issue that will be reviewed is how this can be done in light of the legal requirement that DOE laboratories not compete with work that can be performed by the private sector. These are complicated issues. I believe we can find ways to allow work for others that is within DOE's charter, which would promote the transfer of technology in the private sector, and permit full use of those DOE unique expertise and facilities that are not available from the private sector.

## BASIC RESEARCH

2. DOE is requesting substantial increases for its basic research programs. In part, these increases are required for the construction and operation of large, complex research facilities.

How are you setting priorities for facility needs in each of your basic research activities?

In each of our basic research programs - High Energy Physics, Nuclear Physics, Basic Energy Sciences - we strive for appropriate utilization levels within available funding, to maximize effectiveness and efficiency in utilization of existing facilities. In frontier research, such as conducted in Basic Energy Sciences and High Energy and Nuclear Physics, new facilities must be provided on a timely basis to maintain a forefront position in this frontier research, e.g., new accelerators, advanced light sources.

We have a variety of ways in which we establish the priorities for facility construction. In many cases, we rely heavily on the advice of formal committees drawn from the affected scientific communities. For others, we solicit advice with a variety of less formal mechanisms. In all cases, the judgment of the experienced technical staff of the Department is an essential component of the decision. New facilities are not proposed until appropriate benefit has been achieved from existing expensive Government facilities.

## BASIC RESEARCH

3. With basic research facilities becoming increasingly more complex and expensive, what are the opportunities for international collaboration in the construction and operation of the next generation of facilities, particularly in high energy physics?

What steps are you taking to foster international collaboration and cost-sharing?

There are extensive informal collaborations with Western Europe on use of present and proposed facilities which form a base of experience and a framework for present and future collaborative efforts. The U.S. also has existing formal bilateral agreements with Japan, PRC, and the USSR which provide the basis for most international cooperation with those countries.

High energy physics machines required for forefront research in the next century will probably have costs well in excess of $1 billion and are likely to require resources beyond the capability of a single nation or region. Such facilities will probably require international cooperation for their construction and operation. The U.S. is participating in ICFA (International Committee on Future Accelerators), a subcommittee of the International Union of Pure and Applied Physics, which is presently proposing mechanisms for international coordination of advanced accelerator R&D and working on problems associated with potential large international cooperative ventures.

## BASIC RESEARCH

4. The university research community has expressed dissatisfaction with its limited participation in DOE research activities.

What are you doing to address the concerns of the university research community?

What is DOE's proper responsibility for improving university scientific equipment?

The share of the Office of Energy Research support provided to university scientists has increased over the last three years. The Basic Energy Sciences program provides about 26 percent of its operating budget to university scientists (up from 18 percent in FY 1978). The percentage would increase to 37 percent if support to the Lawrence Berkeley Laboratory (LBL) and Ames Laboratory is included. These two laboratories are closely tied respectively to Berkeley and Iowa State campuses and through the years have provided numerous opportunities for university faculty and students to participate in Laboratory research programs. For example over 500 graduate students are involved each year in programs at LBL alone. In addition, 170 LBL scientists have joint professorial appointments on the Berkeley campus.

We are committed to utilizing effectively university capabilities in support of DOE's missions to look in depth at the university communities alleged dissatisfaction, as well as other related issues. The Deputy Secretary has asked the Energy Research Advisory Board (ERAB) to carry out an assessment of the overall effectiveness of DOE support for university research. The ERAB study panel for this purpose is chaired by Dr. Ivan Bennett of New York University. The panel's report is due to ERAB by this summer. Among the issues being looked at by the panel are the relationships between the universities and the national laboratories; the DOE role in the development of new professional manpower; and the barriers (both actual and perceived) to more effectively utilizing university capabilities in support of the research and technology development programs of the Department.

DOE believes that it is responsible for providing universities the specialized facilities and equipment which they require to conduct the long-range energy research programs sponsored by the Department. During the last few years DOE has devoted substantial funding to develop unique user-oriented research facilities which will be used primarily by university faculty. Such facilities include the National Synchrotron Light Source, the Combustion Research Facility, the Corrosion Research Laboratory, and a high voltage and high resolution electron microscopy facility. About 8 to 10 percent of the operating funds which DOE provides universities for research is used to purchase equipment.

Many university energy research programs also involve large numbers of researchers (both faculty and graduate students) who need continuous access to certain major instruments such as scanning electron microscopes, nuclear magnetic resonance spectrometers, laser diagnostic systems, etc. DOE is planning to meet this need by initiating the University Research Instrumentation program in FY 1984. This program would focus on aiding universities to purchase the large, state-of-the-art instrumentation (generally costing in excess of $100,000 each) which is required for research in specific areas such as combustion, catalysis, radioactive waste management, biological energy conversion, etc. DOE has a special responsibility to ensure that continued progress is made in these specific fields.

DOE's instrumentation program will be part of an interagency effort to improve university scientific equipment. The Department will coordinate its program and awards with the other participating agencies so that the instrumentation provided by each agency will be complementary, prevent duplication of similar equipment and contribute to advancing the research goals of all the agencies.

## BASIC RESEARCH

5. We hear from university representatives that the changes NSF is making in its grants to give grantees clear responsibility to manage research will enhance the effectiveness of the management of research. Would you consider taking the approach NSF has taken?

We, in DOE, have always insisted that our contractors, including universities, be responsible for the management of research.

DOE essentially uses two types of contracts with universities--the Special Research Contract (SRC) for awards of $1 million or less per year and a "normal" cost reimbursable contract when the funding exceeds $1 million per year.

The SRC is a unique document designed to give maximum flexibility to the principal investigator for research management with only minimal administrative approval requirements. For example, the contractor may change the specific objective in the research work and approval by DOE is required only if the contractor proposes a change in the phenomenon or phenomena under study. DOE approval is also required for a change in the principal investigator, the purchase of large dollar value equipment not included in an approved budget, foreign travel and the acquisition of excess personal property. We feel that certain minimum controls are appropriate to ensure that public funds are spent wisely and are convinced that they do not impose an onerous burden on our university contractors. Nevertheless, we do intend to observe NSF's progress in this area.

## BASIC RESEARCH

6. Many university researchers have programs of research that are artifically fragmented by the multiple support of several federal agencies. This interferes with the effective management of research. NSF is trying to deal with this problem by permitting grantees to make determinations that projects are scientifically or technically related and thereafter regard them as a single project for research management purposes. Would you consider joining NSF in doing this, so long as your Program Officers could approve relatedness in each case?

The funding of university research by DOE is usually based upon a proposal received from the university and jointly signed by the proposed principal investigator. If the proposed effort is closely related to DOE-funded work already being undertaken by the principal investigator, the additional effort is normally funded under the existing agreement. To a large extent, any fragmentation is traceable to the number and content of proposals submitted. In general, I am concerned about funds accountability where one or more agencies will be funding closely related work under different award instruments. A more prudent approach is to have one agency support the work under one award instrument or, where multiple agency support is warranted, to have a single instrument between one agency and the university with an interagency transfer of funds from other interested agencies.

## BASIC RESEARCH

7. **There are complaints in the university community that your use of contracts instead of grants for the support of research interferes with the management of research by causing unnecessary delays and proliferation of paper. Why do you use contracts, which are for procuring services for federal use, for the support of university research?**

I am not aware of any reason why the use of a contract, rather than a grant, would cause any delay or proliferation of paper. The DOE lead time for award of a grant or the Special Research Contract that we use with universities are identical. In fact, most of our researchers are unable to detect any differences between contracts and grants and refer to them interchangeably.

Public Law 95-224, the Federal Grant and Cooperative Agreement Act of 1977, requires that the choice and use of legal instruments reflect the type of basic relationship expected between the Federal Government and non-Federal parties. A contract should be used when the activity is principally for the direct benefit or use of the Federal Government; on the other hand, when the the principal purpose of the activity is to accomplish a public purpose of support or stimulation, an assistance instrument--a grant or cooperative agreement--should be used. In most instances in the Office of Energy Research, we consider our principal purpose to be the attainment of energy-related research for use by the Department in the planning of its technology programs and to provide a base for the development and advancement of energy technologies. The High Energy and Nuclear Physics programs are managed by DOE with the goal of maintaining the pre-eminence of the U.S. in these fields. Such research results are used primarily by the Government in our ongoing programs and in planning for the future directions of these programs.

In summary, most of the ER supported research is conducted primarily for the use of the Government in the accomplishment of its assigned mission, and it is thus proper that we use contracts. We are, however, working toward the use of grants, where appropriate, and are in the process of developing procedures which will permit timely award under terms no more burdensome than those for Special Research Contracts.

## BASIC RESEARCH

8. The Office of Energy Research supports work in computational sciences in the Fusion, High Energy Physics, Nuclear Physics and Basic Energy Sciences programs.

Please provide the Committee with the amount of funding in FY 1983 and FY 1984 for computational science from each of these programs.

How is this research coordinated within the Department of Energy and with the research being funded by the National Science Foundation and DOD?

The pure research in computational sciences is strictly the responsibility of the Applied Mathematical Science program within Basic Energy Sciences (BES). The FY 1983 funding for this program is $14.7 million and $15.6 million is requested in the FY 1984 budget. Other applied aspects of computational science pervade other Energy Research programs and DOE programs.

Coordination is effected through colocation of research at DOE laboratories and through interagency committees and personal staff interactions. In addition, research is coordinated through the Interagency Committee on Extramural Mathematics. Also, Proposal List and Project Summaries are exchanged with other Governmental agencies.

## BASIC RESEARCH

9. In the past few years there has been a series of budget structure changes involving the General Science and Research appropriation account. Why were these changes necessary?

Does the FY 1984 budget structure represent a stable arrangement, at least for the next few fiscal years?

An FY 1983 structure change was made to fund low energy nuclear physics activities in the Nuclear Physics program rather than in the Basic Energy Sciences program. The purpose of this change was to align budget responsibility with the Low Energy Nuclear Physics program management responsibility which resides in the Nuclear Physics program.

Additional FY 1983 structure changes were made regarding BEVALAC, the High Flux Beam Reactor (HFBR) and the High Flux Isotope Reactor (HFIR) funding responsibility. These changes were made to align total facility funding responsibility in a single program or subprogram in accordance with prudent management practices.

An FY 1984 structure change was made to merge the Life Sciences Research and Nuclear Medicine Applications (LSRNMA) program into the Biological and Environmental Research (BER) program. The purpose of this change was to eliminate management confusion since both the LSRNMA and BER programs are managed by the same staff and are so complimentary and interrelated that budgeting for them separately is confusing, arbitrary, and inefficient.

We believe the FY 1984 structure does represent a stable structure for the forseeable future. Structure changes will only be proposed when necessary due to management changes or changes in the nature of a program such as graduating from the research phase to a technology application phase.

## BASIC ENERGY SCIENCES

1. The Basic Energy Sciences programs in Materials Sciences, Chemical Sciences and Nuclear Sciences support several large facilities at the national laboratories. The operating expenses for each of these programs can be divided into funds which support research at the national laboratory facilities and funds which support research elsewhere.

   For each of these programs, please provide a table that indicates the operating expenses for each major facility in the program and the operating expenses for all other activities in the program for FY 1983 and FY 1984.

   In the category of other research in these programs, please indicate the amount going directly to universities.

I will be happy to provide for the record a table with the information requested. For the record let me also make a couple of points about the information I will be providing. The amount which I show for facilities operations support provide benefits not only to the laboratory research programs but also provides a good deal of benefit to the university community. For some facilities the level of university activity is upwards of 50 percent of the research conducted. For facility operations in FY 1984 we project that around $11.0 million of amount shown is of direct benefit to the university community. In addition the Stanford Synchrotron Radiation Laboratory is operated by Stanford University and we are providing $6.8 million of support in FY 1984. For FY 1984 therefore, we project that the BES program provides about $84 million of support to the university community. In addition, two of our major laboratories, Lawrence Berkeley Laboratory and Ames Laboratory, are located at university campuses and therefore also provide benefit to the university community.

## BASIC ENERGY SCIENCES—OPERATING EXPENSES
Research by Activities at Facilities and Universities
(Dollars in millions)

| | FY 1983 | | | | | FY 1984 | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | Fac. Ops. | Lab. Res. at Fac. | Univ. Res. | All Other | Total Funding | Fac. Ops. | Lab. Res. at Fac. | Univ. Res. | All Other | Total Funding |
| MATERIALS SCIENCES | | | | | | | | | | |
| Major Facilities: | | | | | | | | | | |
| National Synchrotron Light Source (NSLS)..................... | $ 6.0 | $ 3.5 | $ 1.8 | $ - | $ 11.3 | $ 7.2 | $ 4.0 | $ 2.3 | $ - | $ 13.5 |
| High Flux Beam Reactor (HFBR)..... | 6.3 | 2.0 | - | - | 8.3 | 6.6 | 2.1 | - | - | 8.7 |
| Intense Pulsed Neutron Source (IPNS)..................... | 4.6 | 2.3 | - | - | 6.9 | 4.9 | 2.4 | - | - | 7.3 |
| High Voltage Electron Microscopes (HVEM) & Atomic Resolution Microscopes (ARM)................. | 1.5 | 1.5 | 0.2 | - | 3.2 | 1.7 | 1.8 | 0.3 | - | 3.8 |
| Weapons Neutron Research Facility (WNR).................... | - | 1.4 | - | - | 1.4 | - | 1.6 | - | - | 1.6 |
| High Flux Isotope Reactor (HFIR).. | - | 2.0 | - | - | 2.0 | - | 2.1 | - | - | 2.1 |
| All Other Research................. | - | - | 19.2 | 56.4 | 75.6 | - | - | 19.8 | 68.3 | 88.1 |
| Total Materials Sciences............ | 18.4 | 12.7 | 21.2 | 56.4 | 108.7 | 20.4 | 14.0 | 22.4 | 68.3 | 125.1 |
| CHEMICAL SCIENCES | | | | | | | | | | |
| Major Facilities: | | | | | | | | | | |
| Combustion Research Facility (CRF) | 2.2 | 2.6 | - | - | 4.8 | 2.5 | 2.8 | - | - | 5.3 |
| National Synchrotron Light Source (NSLS)..................... | 3.4 | 1.4 | - | - | 4.8 | 3.9 | 1.9 | - | - | 5.8 |
| High Flux Beam Reactor (HFBR)..... | - | 0.8 | - | - | 0.8 | - | 0.9 | - | 0 | 0.9 |
| All Other Research................. | - | - | 17.5 | 42.1 | 59.6 | - | - | 18.7 | 44.8 | 63.5 |
| Total Chemical Sciences............. | 5.6 | 4.8 | 17.5 | 42.1 | 70.0 | 6.4 | 5.6 | 18.7 | 44.8 | 75.5 |
| NUCLEAR SCIENCES | | | | | | | | | | |
| Major Facilities: | | | | | | | | | | |
| High Flux Isotope Reactor (HFIR).. | 5.7 | 2.3 | - | - | 8.0 | 6.2 | 2.6 | - | - | 8.8 |
| Transuranium Processing Plant (TRU)......................... | 4.8 | - | - | - | 4.8 | 5.1 | - | - | - | 5.1 |
| Calutrons.......................... | 0.5 | - | - | - | 0.5 | 0.5 | - | - | - | 0.5 |
| Mound.............................. | 0.1 | - | - | - | 0.1 | 0.1 | - | - | - | 0.1 |
| Stanford Synchrotron Radiation Laboratory (SSRL)................. | 6.4 | - | - | - | 6.4 | 6.8 | - | - | - | 6.8 |
| Oak Ridge Electron Accelerator (ORELA)......................... | 1.5 | 1.5 | - | - | 3.0 | 1.6 | 1.6 | - | - | 3.2 |
| All Other Research................. | - | - | 1.5 | 6.3 | 7.8 | - | - | 1.2 | 12.2 | 13.4 |
| Total Nuclear Sciences.............. | 19.0 | 3.8 | 1.5 | 6.3 | 30.6 | 20.3 | 4.2 | 1.2 | 12.2 | 37.9 |
| All Other Basic Energy Sciences..... | - | - | 21.6 | 30.4 | 52.0 | - | - | 24.0 | 33.3 | 57.3 |
| Total Basic Energy Sciences......... | $ 43.0 | $ 21.3 | $ 61.8 | $135.2 | $261.3 | $ 47.1 | $ 23.8 | $ 66.3 | $158.6 | $295.8 |

## BASIC ENERGY SCIENCES

**2. In Basic Energy Sciences, how do you determine which subprograms have management responsibilities for which major facilities?**

**Specifically, the Stanford Synchrotron Radiation Laboratory is in the nuclear sciences subprogram budget, whereas the National Synchrotron Light Source is managed by the Materials Sciences program, with operating funding from both Materials Sciences and Chemical Sciences. How do you rationalize this arrangement?**

Because the condensed matter community expressed the strongest need for a new synchrotron light facility, the National Synchrotron Light Source Facility (NSLS) was proposed as a Materials Sciences construction project. Materials Sciences continues to exercise managerial responsibility for the facility. When the project progressed from the construction phase to the operating phase, it was decided to split the facility operating costs between Materials and Chemistry because of the projected strong utilization by both programs and the desire to budget on the basis of where the research is being conducted. Facility support for NSLS is closely coordinated by the two subprograms and overseen by Basic Energy Sciences management.

The Stanford Synchrotron Radiation Laboratory (SSRL) was administratively placed in the Nuclear Sciences subprogram because of the close coordination betwen the Nuclear Sciences subprogram and the High Energy and Nuclear Physics (HENP) program. The SSRL uses radiation produced by the Stanford Positron Electron Asymmetric Ring (SPEAR) of the Stanford Linear Accelerator Center (SLAC), an HENP facility.

## BASIC ENERGY SCIENCES

3. The budget request indicates that the operating expenses associated with the National Center for Advanced Materials (NCAM) includes $4.5 million for a forefront research program.

Will all these funds be expended at the Lawrence Berkeley Laboratory in FY 1984?

What funds will be available to assist other users to prepare themselves for the NCAM facilities?

The funds will be expended primarily at Lawrence Berkeley Laboratory (LBL), for graduate students and faculty at the University of California, Berkeley. These funds include support for workshops to help guide the eventual research program and for some visiting scientists to begin the research program. Some funds will also be used to carry out research at the Stanford Synchrotron Radiation Laboratory (SSRL). Work will begin on surface science and catalysis ($1,100,000), advanced materials synthesis ($1,400,000) and advanced device concepts ($600,000). The engineering group at the University of California, Berkeley, not now associated with LBL, will be brought into the program. In addition to the operating funds, equipment will be provided for the Surface Science and Catalysis Laboratory (SSCL) ($400,000), Advanced Materials Synthesis Laboratory (AMSL) ($700,000) and Advanced Device Concepts Laboratory (ADCL) ($300,000).

It is anticipated that workshops and visiting scientists will be supported. Proposals for use of the facilities when completed will also be supported for those scientists who do not have their own support. In future years, teams of researchers from outside LBL will be supported to carry out research at the National Center for Advanced Materials (NCAM). The operation of the NCAM facilities is funded by the Materials Sciences subprogram. This will make it convenient to use the facility by all qualified scientists in a manner similar to that at existing facilities.

## BASIC ENERGY SCIENCES

4. Please compare the proposed NCAM synchrotron radiation facilities at LBL and SSRL with the proposed upgrade to NSLS -- with respect to energy, brilliance, and other relevant physical properties.

   What evidence does the Department have that substantiates the need for a significant new synchrotron radiation center?

   I will be happy to provide the requested information for the record.

## INSERT FOR THE RECORD

Lawrence Berkeley Laboratory (LBL)

Design, based on exclusive use of insertion devices rather than bending magnets, pushes state-of-the-art to achieve:

- spectral brilliance several orders of magnitude higher than any existing machine [including National Synchrotron Light Source Facility (NSLS)].
- time structure with photon pulses about two orders of magnitude shorter in duration for dynamic physical and chemical studies and investigation of excited state decay processes.
- optimum performance in vacuum ultraviolet and soft x-ray spectral regions (about 0.1-10keV).

Stanford Synchrotron Radiation Laboratory (SSRL) (upgrade)

Greatly improved performance in hard x-ray region, with order of magnitude increase in brilliance from bending magnets and achievement of spectral brilliance in 10 to 30 keV range, using insertion devices at the Stanford Positron-Electron Asymmetric Ring (SPEAR) and the Positron-Electron Joint Project (PEP), exceeding any present machine (including NSLS). Work on insertion devices closely coupled with LBL effort.

NSLS improvements aimed primarily at providing additional beamline development and supporting space to take care of a much more rapid buildup of users than anticipated, and to achieve improved performance through the use of insertion devices. The x-ray ring wiggler will provide a unique capability in the very hard (up to 100 keV) x-ray region.

All the synchrotron laboratory R&D will be coordinated with an interlaboratory committee so that each facility can concentrate on maximizing their unique capabilities.

Not only will the Advanced Light Source be a new source, but it will be an integral part of a comprehensive research program , National Center for Advanced Materials (NCAM), and it will take into account all the developments elsewhere to build the most advanced (e.g., all insertion device) facility. This will enable new research areas to be explored and keep the United States in the lead for this activity. It must be recognized that the lead time for these facilities is of the order of 5-8 years.

A recent report by a subcommittee of the National Academy of Sciences, Solid State Sciences Committee reviewed the status of synchrotron radiation facilities in the United States as well as in Europe and Japan. This report revealed that even with all experimental stations in the United States developed, the demand for use would exceed the supply by 1984. The annual rate of growth is 30 percent, and the rate appears to be growing.

The development of insertion devices (wigglers and undulators) has provided orders of magnitude brighter sources of radiation and will make possible new experiments hitherto not feasible or even conceived just a few years ago. The Advanced Light Source (ALS) is designed to be an all-insertion device machine which pushes the current technology to the maximum extent.

Advances in spectral brilliance and intensity, it is seen, will have a significant impact on many technologies such as photolithography, catalysis, medical in-vivo imaging, and others, requiring radiation of a very short time scale (e.g., x-ray holography). The light source at LBL is an integral part of the NCAM where these new technologies will be explored and interaction with industry will take place.

## BASIC ENERGY SCIENCES

5. The continuing operating expenses for the current program at the Stanford Synchrotron Radiation Laboratory (SSRL) are included in the Nuclear Sciences program. In FY 1984 an SSRL upgrade is also included as part of the NCAM project.

Please provide the total operating and construction expense budget to be provided to SSRL in FY 1984.

Now that the transfer of SSRL to DOE is complete, please provide the Committee with any plans to increase the SSRL support staff. How is this reflected in the 1984 budget, as compared to FY 1983?

The FY 1984 Congressional budget includes $6,790,000 in operating expenses in the Nuclear Sciences subprogram, $490,000 in capital equipment in the Nuclear Sciences subprogram, and $4,100,000 in construction funds in the Materials Sciences subprogram for support of the SSRL facility. The construction funds, $4,100,000 in FY 1984, are included as part of the National Center for Advanced Materials and the TEC of the SSRL part of the project is $13,000,000.

Staffing increases were provided for this facility in FY 1983. An additional small increase in research is anticipated in FY 1984.

## BASIC ENERGY SCIENCES

6. With respect to the Combustion Research Facility at Sandia Livermore, please provide a table which indicates total support for the facility available in FY 1982, FY 1983 and FY 1984.

Please indicate the funds available to CRF from the Basic Energy Sciences program, the Fossil Energy program, and the Conservation program.

I will be happy to provide for the record a table with the information you requested.

] INSERT FOR THE RECORD [

The following table reflects the support provided by the Basic Energy Sciences program and the estimates we have received from the Assistant Secretaries for Fossil and Conservation programs.

| | FY 1982 | FY 1983 | FY 1984 |
|---|---|---|---|
| Basic Energy Sciences Program | $ 4.4 | $ 4.8 | $ 5.3 |
| Fossil Energy Program | 1.9 | 2.0 | 2.5 |
| Conservation Program | 1.3 | 1.7 | 1.9 |
| Total | $ 7.6 | $ 8.5 | $ 9.7 |

## BASIC ENERGY SCIENCES

7. The heavy ion fusion activity will be conducted by Lawrence Berkeley Laboratory and Los Alamos National Laboratory; please provide the funds available to each in FY 1984.

Pending revised proposals expected from the laboratories, the following preliminary estimates are provided. Lawrence Berkeley Laboratory would be expected to receive $3.2 million for heavy ion fusion research. Los Alamos National Laboratory would be expected to have $1.6 million, of which a portion might be provided to subcontractors. We are estimating a total of $0.2 million of capital equipment funds will be provided for the Heavy Ion Fusion Program managed by the Office of Energy Research.

## BASIC ENERGY SCIENCES

8. At DOE's request, the National Academy of Sciences has conducted special studies on the needs in applied mathematical sciences.

Please provide those studies to the Committee.

The National Academy's Committee on Applications of Mathematics has been carrying out studies beginning in 1981 on DOE needs in applied mathematical sciences. The chairman is Werner C. Rheinboldt of the University of Pittsburgh. A report is in preparation with completion scheduled for this summer. We will send you a copy of the report as soon as it is available.

## NUCLEAR PHYSICS

1. In FY 1984, $153 million is requested for nuclear physics.

By facility, please provide the operating funds required for each of the national laboratory facilities and the four university laboratories in the program.

Please distinguish between expenses to maintain and operate the facility and expenses for associated research and development.

Also, please provide a similar breakout for FY 1983.

I will be pleased to provide this information for the record.

INSERT FOR THE RECORD

## Nuclear Physics Facilities

Seven accelerator facilities are operated as national facilities whose beam time is made available to all the Nation's scientists on the basis of scientific merit and technical feasibility. The national facilities are: the Clinton P. Anderson Meson Physics Facility (LAMPF) at Los Alamos National Laboratory (LANL); the William H. Bates Linear Accelerator Facility at Massachusetts Institute of Technology (MIT); the SuperHILAC/ Bevalac at Lawrence Berkeley Laboratory (LBL); the Holifield Heavy Ion Research Facility at Oak Ridge National Laboratory (ORNL); the 88-inch Cyclotron at Lawrence Berkeley Laboratory; the Tandem/Linac Facility at Argonne National Laboratory (ANL); and the Double MP Tandem Van de Graaff facility at Brookhaven National Laboratory (BNL). Nuclear Physics directly supports 75 university-based user groups who conduct most or all of their research at these national facilities. To complement the national facilities, the Nuclear Physics program also supports dedicated on-campus accelerators at Yale University, Duke University, University of Washington, and Texas A&M University.

Operating Expenses
(B/A in millions of dollars)

| | FY 1983 | | FY 1984 | |
|---|---|---|---|---|
| National Facilities | Facility Operations | Research & Development | Facility Operations | Research & Development |
| LAMPF | $26.5 | $ 6.6 | $30.8 | $ 7.2 |
| Bates | 4.8 | 1.7 | 5.3 | 2.2 |
| Bevalac | 14.5 | 4.1 | 16.6 | 5.7 |
| Holifield | 4.1 | 3.2 | 4.7 | 3.4 |
| 88-Inch Cyclotron | 2.0 | 2.4 | 2.2 | 2.5 |
| ANL Tandem/Linac | 1.5 | 2.0 | 2.0 | 2.5 |
| BNL Double MP | 1.3 | 1.1 | 1.5 | 1.3 |
| Subtotal | $54.7 | $21.1 | $63.1 | $24.8 |
| Dedicated On-Campus Facilities | | | | |
| Yale U. Tandem | $ 1.0 | $ 0.9 | $ 1.1 | $ 1.0 |
| Duke U. Cyclograaff | 0.5 | 0.6 | 0.7 | 0.7 |
| U. of Wash. Double FN | 0.6 | 0.9 | 0.7 | 1.0 |
| Texas A&M U. Cyclotron | 0.6 | 0.4 | 0.7 | 0.5 |
| Subtotal | $ 2.7 | $ 2.8 | $ 3.2 | $ 3.2 |
| Total | $57.4 | $23.9 | $66.3 | $28.0 |

This table does include support of "in-house" research conducted by scientists based at the host institution but does not include support of research conducted by outside user groups.

## NUCLEAR PHYSICS

2. The FY 1984 budget contains $8 million for upgrades of the accelerators at the University of Washington and Yale University; the total cost of those upgrades is $19 million.

   Please provide the operations requirements for each facility in 1984 and succeeding years.

In FY 1984, there will be no incremental operating expenses associated with these accelerator upgrades since the projects will be in early stages of construction. However, in FY 1985, $0.25 million of incremental operating expenses is required for each university facility. In FY 1986 and beyond, $0.5 million per year of incremental operating expenses is needed for each university facility. The incremental operating expenses include both incremental facility operations costs and incremental research costs.

## NUCLEAR PHYSICS

3. What is the long range DOE commitment for constructing, maintaining and operating university based facilities for nuclear physics research?

Work at university-based facilities is an essential component of the overall DOE basic nuclear research program. We have a strong commitment to maintain the scientific strength of these research programs.

On a long term basis, maintenance of the capacity to execute forefront nuclear research at a university-based facility requires periodic upgrade of the accelerators themselves as well as associated instrumentation. In our FY 1984 Request, such improvement of on-campus accelerator facilities at Yale University and the University of Washington is planned. The upgraded facilities at Yale and Washington will allow continuation of research programs which are scientifically productive and which provide graduate students with training at the frontiers of basic nuclear research. In terms of scientific accomplishment and training of scientists, Yale and Washington are each leading American educational institutions in the field of basic nuclear research.

## NUCLEAR PHYSICS

4. Does the FY 1984 Nuclear Physics Construction request of $13.7 million represent the planned level of annual construction for this program over the next several years?

What is the appropriate percentage of total nuclear physics funding that should be invested in construction of new facilities?

A Long Range Plan for Nuclear Science, developed by the DOE/NSF Nuclear Science Advisory Committee (NSAC) in 1979, recommends that an average of 12% of the total basic nuclear physics budget be invested in construction; the $13.7 million construction request for FY 1984 is 9% of the total Nuclear Physics budget. The NSAC Plan has been adopted by DOE and the National Science Foundation (NSF) as a framework for coordinated management of DOE-supported and NSF-supported work in basic nuclear research.

The basic nuclear research community is now actively discussing scientific opportunities and options which, if implemented, would require an increased rate of facility construction over the next several years. Success of basic nuclear research depends upon the existence and effective operation of accelerator facilities.

## NUCLEAR PHYSICS

5. What are the highest priority construction items for future years (FY 1985-87)?

In the near term (FY 1985-87), the highest priority for new facility construction in basic nuclear research is the establishment of a high energy, high duty factor electron accelerator capability. Such a facility is viewed by the scientific community and the Department as the next logical step for advancement of basic nuclear research in the United States. Over the longer term, there is a growing scientific interest in establishment of new nuclear research capability with high energy heavy ions and with high intensity K meson, neutrino, and antiproton beams.

## NUCLEAR PHYSICS

6. Do you anticipate any significant change in the scope of the DOE nuclear physics program (for example, the balance between the "medium energy" and "heavy ion" activities)?

The FY 1984 Nuclear Physics Request provides for a well-balanced program of basic nuclear research. Thus, in the near term, we do not plan a significant change in balance between medium energy and heavy ion activities. Over the longer term, the DOE Nuclear Physics program balance must change to accommodate emerging scientific opportunities.

## NUCLEAR PHYSICS

7. In FY 1984 the Bevalac biomedical activity will be funded directly in the heavy ion nuclear physics budget. Will future biomedical research at nuclear physics facilities be supported directly by the nuclear physics program?

The Nuclear Physics program supports 100% of Bevalac facility operations costs. However, direct support of biomedical research (i.e., the experiments themselves as distinguished from the cost of providing beam) remains in FY 1984 the responsibility of the National Cancer Institute (NCI) and the DOE Biological and Environmental Research (BER) program. We expect this pattern of Bevalac facility operations support by Nuclear Physics and biomedical research support by NCI and BER to continue. We have no plans for Nuclear Physics to support biomedical research other than by provision of beams which are uniquely available at Nuclear Physics accelerator installations.

## HIGH ENERGY PHYSICS

1. What is the significance to the U.S. High Energy Physics Program of the reported discovery of the W particle at CERN?

Two of the four fundamental forces of nature - the weak nuclear and the electromagnetic interactions - have been "unified" into a single electroweak interaction in a recently developed theoretical framework. A major prediction of this unified theory is the existence of the W particle, the carrier of the weak nuclear force, and its mass. The CERN observations are the first direct confirmation of the existence of the W particle, and the preliminary analysis appears to be consistent with the predicted mass. This is a strong indication that the ultimate goal of the unification of all physical forces into a single basic interaction may be within reach. These experimental results also provide very important input for the planning of new facilities and experiments. Future and planned new facilities at SLAC, Fermilab and BNL will enable us to study detailed properties of the W particles and its "partner", the Z° particle, which is also predicted by the unified theory.

## HIGH ENERGY PHYSICS

2. To what extent are the current U.S. and CERN facility construction programs complimentary or duplicative?

The current DOE/HEP construction projects and planned facilities are unique and complementary to those at the European Organization for Nuclear Research (CERN). In the area of Fixed Target Proton Accelerators, the existing CERN Super Proton Synchrotron gives primary protons with an energy of 400 GeV while the Energy Saver/Tevatron II upgrading at Fermi National Accelerator Laboratory, which introduces the use of superconducting magnets in particle accelerators, provides proton beams with an energy of 1000 GeV.

In the area of Proton-Antiproton Colliders, CERN has achieved energy of 540 GeV in the center of mass and a luminosity (reaction rate) of 5 x $10^{28}$ $cm^{-2}$ $sec^{-1}$ while the Fermilab Tevatron I Project will provide an energy of 2000 GeV center of mass energy and a luminosity of at least $10^{30}$ $cm^{-2}$ $sec^{-1}$.

For Proton-Proton Colliders the Intersecting Storage Ring (ISR), which has been in operation for a decade at CERN, reaches 62 GeV center of mass energy while the proposed colliding beam accelerator (CBA) project at Brookhaven would have an energy of 800 GeV center of mass energy and a peak luminosity of $10^{33}$ $cm^{-2}$ $sec^{-1}$.

For Electron-Positron Colliders, the first phase of the Large Electron Positron project under construction at CERN will give greater than 100 GeV center of mass energy with a luminosity of approximately $10^{31}$ $cm^{-2}$ $sec^{-1}$ while the SLAC Linear Collider (SLC) will produce greater than 100 GeV center of mass energy with an entirely new accelerator technology which holds the promise of lower cost and better performance for future very high energy electron-positron colliders. Also, SLC will reach a high luminosity to allow definitive study of the Z° particle.

## HIGH ENERGY PHYSICS

3. Is there parity between U.S. researchers using foreign HEP facilities and foreign research teams using U.S. HEP facilities?

Yes, there has been parity averaged over the years. The HEP facilities throughout the world are essentially unique and complementary. To pursue a full program of research it is essential that U.S. physicists have access to the unique foreign facilities. All HEP facilities are open to qualified users on the basis of scientific merit of their proposals. I am happy to report that this point of view is reciprocated throughout the world. For example U.S. scientists, supported by DOE, participated in the recent experiment in which the very important discovery of the W particle was made. U.S. physicists have the lead role in one of the major experiments at the European large electron proton facility and are participants in three others. Japanese and Italian scientists are playing a significant role in the fabrication (and funding) of the Collider Detector Facility at Tevatron I at Fermilab.

## HIGH ENERGY PHYSICS

4. Please provide the committee with FY 1984 and outyear DOE funding for Dr. Ting's detector at CERN/LEP.

We are pleased that Dr. Ting, the scientific group leader, and his U.S. and foreign collaborators now have an approved experimental detector, named L3, on the European Organization for Nuclear Research (CERN)/Large Electron Positron (LEP) facility. The L3 collaborative effort also involves about $23 million of contributions from eight other countries. These countries include France, Germany, Italy, Spain, Switzerland, the Netherlands, the USSR, and the Peoples Republic of China. The U.S. universities now participating are: California Institute of Technology, Carnegie-Mellon, Hawaii, Harvard, Massachusetts Institute of Technology, Ohio State, Oklahoma, Princeton and Yale.

The FY 1983 and outyear DOE projections for L3 are: FY 1983, $2.0M; FY 1984, $3.0M; FY 1985, $5.0M; FY 1986, $5.0M; FY 1987, $3.0M and FY 1988, $2.0M.

## HIGH ENERGY PHYSICS

5. Please provide the Committee with FY 1984 and outyear foreign funding for detectors and other equipment at U.S. facilities.

There are several different ways in which foreign institutions contribute to the financing of high energy physics research activities at U.S. facilities. There are direct transfers of funds which are spent in this country to acquire or fabricate detectors and other research equipment, and there are contributions or long-term loans of foreign built or purchased components and detector subsystems. Foreign research groups also usually provide salaries, subsistence and travel expenses for their scientists, students, and technicians who are temporarily assigned to a U.S. facility while working on a given research project.

I will provide for the record estimates of the amounts associated with these activities for FY 1984 and outyears. I think they will probably total between ten and fifteen million dollars per year.

## INSERT FOR THE RECORD

| Category | FY 1984 | Outyears |
|---|---|---|
| Direct transfer of foreign funds | $5,500,000 | No significant change anticipated |
| Contribution or long term loan of foreign built equipment | $3,500,000 | Slight decrease foreseen |
| Salary and travel support for foreign personnel working in U.S. | $5,000,000 | Slight decrease foreseen |

## HIGH ENERGY PHYSICS

6. What are the major milestones during FY 1983 and FY 1984 leading to a decision on the Colliding Beam Accelerator facility at Brookhaven?

BNL is preparing a new proposal for a proton-proton colliding beam facility to build on the existing investment at BNL and their R&D results to date. This will provide a candidate for the major physics capability needed by the end of the 1980's, as recommended by the 1981 High Energy Physics Advisory Panel (HEPAP) Subpanel on Long Range Planning. This proposal will be received by DOE on March 30, 1983. We have scheduled an indepth technical, cost, and schedule review of this proposal by DOE in April 1983.

HEPAP has formed a 1983 New Facilities Subpanel chaired by Professor Stan Wojcicki. This Subpanel had its organizational meeting on February 25 and 26 and is scheduled to complete its report by mid June. HEPAP will review their report at its meeting on June 30 and forward the report to DOE with its own recommendations at that time. The Colliding Beam Accelerator is clearly one of the main issues facing this Subpanel.

After these reviews are complete, DOE will make a decision with regard to future facility construction at BNL and the FY 1985 budget.

## HIGH ENERGY PHYSICS

7. In FY 1984 how much high energy physics funding would support non-accelerator experiments?

Do you anticipate this component of the DOE program to grow as a percentage of the total program?

The FY 1984 high energy funding in this category is estimated to be some $7M. It supports such research projects as: proton decay, monopole searches, quark searches and ultra high energy cosmic ray experiments.

We do not anticipate any substantial growth in this component of the DOE program.

## HIGH ENERGY PHYSICS

8. By what process are you planning for the next generation HEP facilities (beyond the Tevatron complex, SLC and CBA)?

Preliminary study and consideration of the physics and facilities required for the future progress of this leading field of basic research were made during the three-week 1982 Snowmass/Aspen Summer Study sponsored and organized by the Division of Particles and Fields (DPF) of the American Physical Society. The DPF is also conducting a one-week study on "detectors for high luminosity operation" at LBL in February-March 1983. A one-week workshop focusing on "advanced accelerator R&D needed for the next generation facilities" will be held at Cornell University in March 1983. Detailed consideration of new facilities and accelerator R&D needs is the priority task of the 1983 High Energy Physics Advisory Panel Subpanel on New Facilities for High Energy Physics. It is scheduled to report its recommendations to HEPAP in June 1983. DOE anticipates future Snowmass-type summer studies and subsequent long range planning panels' review and study.

DON FUQUA (Fla.), Chairman

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Executive Director

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General Counsel

DAVID S. JEFFERY
Minority Staff Director

March 21, 1983

Alvin Trivelpiece, Director
Office of Energy Research
Department of Energy
Washington, D.C. 20515

Dear Dr. Trivelpiece:

Thank you for your excellent testimony before the Subcommittee on Energy Development and Applications on February 24. During the hearing you indicated that you would respond to additional questions submitted in writing. I would appreciate your response to the two questions that are attached.

I would appreciate your response to the first question by the close of business on March 22, and your response to the second question as soon as the data are available.

Thank you for your cooperation.

Sincerely,

Don Fuqua

DON FUQUA
Chairman

DF:Hcw
attachment

## QUESTIONS FOR DR. TRIVELPIECE

1. Is there a clearly established, yet unmet, demand for state-of-the-art computational capabilities to solve problems in physics, chemistry and mathematics within the DOE's energy research programs?

   If so, how do you propose to address the need for advanced computational capabilities in FY 1983 and FY 1984?

   If a fully independent advanced computational program were to be initiated in FY 1984, what additional funding would be required?

2. DOE has requested $30.8 million in the Nuclear Physics program for FY 1984 accelerator facility operations at the LAMPF facility, Los Alamos National Laboratory. DOE projects that $30.8 million in FY 1984 will allow LAMPF to operate approximately 3000 beam hours (1982 utilization level) for research.

   Please provide the Subcommittee with the DOE calculations, including inflation rate guidance, that leads to $30.8 million for 3000 FY 1984 beam hours.

   Please provide the Subcommittee with the calculations to achieve 3000 FY 1984 beam hours using the Los Alamos Laboratory's overhead and inflation rates.

   How much additional funding would be required to achieve 3000 beam hours if the Los Alamos-based calculations are used?

   Assuming that LAMPF accelerator operations are to be maintained at 3000 beam hours in FY 1984, how do you propose to reconcile the difference between the DOE and Los Alamos calculations?

Department of Energy
Washington, D.C. 20585

March 24, 1983

Honorable Don Fuqua
Chairman, Subcommittee on Energy
Development and Applications
Committee on Science and Technology
House of Representatives
Washington, D.C. 20515

Dear Mr. Chairman:

Following the February 24, 1983, hearing before the Subcommittee on Energy Development and Applications, of the Committee on Science and Technology, you sent sent two questions to Dr. Alvin Trivelpiece with your letter dated March 21, 1983.

Enclosed is the answer to question 1. The other response will be sent as quickly as possible.

If you have any questions, please call Ingrid Nelson or Tom Pretorius of my staff on 252-4277. They will be happy to assist you.

Sincerely,

Thomas H. Pretorius
for Robert G. Rabben
Assistant General Counsel
for Legislation

Enclosure

Question 1:

Is there a clearly established, yet unmet, demand for state-of-the-art computational capabilities to solve problems in physics, chemistry and mathematics within the DOE's energy research programs?

If so, how do you propose to address the need for advanced computational capabilities in the FY 1983 and FY 1984?

If a fully independent advanced computational program were to be initiated in FY 1984, what additional funding would be required?

Answer:

There is a large, unsatisfied demand for state-of-the-art, large-scale (Class VI) computational capabilities to solve important problems in physics, chemistry and mathematics within DOE's energy research programs. At the present time, very few scientists in the Energy Research programs, with the except of Magnetic Fusion, have access to Class VI machines. A recent study conducted for the Office of Energy Research estimates a suppressed demand for at least one Class VI computer.

For FY 1983 and FY 1984, we will not have the computing facilities to satisfy the Class VI computing requirements of the Energy Research programs. However, as an interim solution, I have assigned approximately five percent, 800 hours/year, of the Class VI time on the Magnetic Fusion Energy network to the remainder of the ER programs. This time will be reimbursed by the user programs. By utilizing the existing network, we are able to initiate service rapidly and provide state-of-the-art interactive computing on the most advanced scientific computers available not only to the DOE laboratories but to universities and industrial contractors as well. Time will be allocated based on the scientific merits of the problems to be addressed.

We are currently examining alternatives for more adequately meeting the computing requirements of the Energy Research programs. An optimum solution would likely be based on a centralized computing facility and a communications network to service our many geographically dispersed contractors. Based on our experience with the Magnetic Fusion Energy network, we estimate an annual operating cost of $11 - $13 million, including the lease of a Class VI computer, and a capital equipment budget of about $2 million. The actual costs would depend on siting of the computing center, configuration of the network and details of the services to be provided.

**Department of Energy**
**Washington, D.C. 20585**

April 4, 1983

Honorable Don Fuqua
Chairman, Subcommittee on Energy
Development and Applications
Committee on Science and Technology
House of Representatives
Washington, D.C. 20515

Dear Mr. Chairman:

On March 24, 1983, we sent you the answer to question 1 which you had submitted to Dr. Alvin Trivelpiece following the February 24, 1983, hearing before the Subcommittee on Energy Development and Applications. Enclosed is the answer to question 2,submitted in connection with the same hearing, which completes our response.

If you have any questions, please call Ingrid Nelson or Tom Pretorius of my staff on 252-4277. They will be happy to assist you.

Sincerely,

Thomas H Pretorius

Robert G. Rabben
Assistant General Counsel
for Legislation

Enclosure

bc: Ms. Christine Wegman
Subcommittee Staff

## NUCLEAR PHYSICS

2. DOE has requested $30.8 million in the Nuclear Physics program for FY 1984 accelerator facility operations at the LAMPF facility, Los Alamos National Laboratory. DOE projects that $30.8 million in FY 1984 will allow LAMPF to operate approximately 3000 beam hours (1982 utilization level) for research.

Please provide the Subcommittee with the DOE calculations. Including inflation rate guidance, that leads to $30.8 million for 3000 FY 1984 beam hours.

Please provide the Subcommittee with the calculations to achieve 3000 FY 1984 beam hours using the Los Alamos Laboratory's overhead and inflation rates.

I will provide this information for the record.

### DOE Calculations

At the time the FY 1984 Budget Request was formulated, there existed significant uncertainties in FY 1983 funding levels and thus FY 1983 levels of facility utilization. Because of this situation, FY 1982 (rather than FY 1983) was used as the base year for projection of FY 1984 activities. Our projections of FY 1984 LAMPF facility operations were made by consideration of three factors: (1) actual FY 1982 LAMPF facility operations costs; (2) actual FY 1982 LAMPF beam hours; and (3) an estimate of escalation indices from FY 1982 to FY 1984. The specific indices used were based upon guidance from the Office of Management and Budget, in particular, +5.6% from FY 1982 to FY 1983 and +5.5% from FY 1983 to FY 1984. When compounded, these indices imply +11.4% from FY 1982 to FY 1984.

LAMPF Facility Operations
(Dollars in Thousands)

| | Actual FY 1982 | | DOE Request FY 1984 | |
|---|---|---|---|---|
| | B/A | B/O | B/A | B/O |
| Nuclear Physics | $24,272 | $25,605 | $30,800 | $30,800 |
| Defense Programs | 2,000 | 2,000 | * | * |
| | $26,272 | $27,605 | $30,800<br>(+17.2%) | $30,800<br>(+11.6%) |
| Beam Hours | 3095 | | 3000 | |

* No funds specifically identified in the FY 1984 Budget Request.

Based upon the above funding table and escalation indices, we projected that LAMPF would operate in FY 1984 about 3000 beam hours.

## Los Alamos Calculations

The most recent information provided to us by Los Alamos National Laboratory projects, for nonelectric power components of Laboratory activities, composite escalation indices of +7.5% from FY 1982 to FY 1983 and +10.6% from FY 1983 to FY 1984. When compounded, these nonelectric power indices imply +18.9% from FY 1982 to FY 1984. Los Alamos estimates that LAMPF electric power costs per kilowatt-hour will increase by 35.6% from FY 1982 to FY 1984.

LAMPF Facility Operations
(Dollars in Thousands)

| | Actual FY 1982 | | Estimate Based on LANL Indices FY 1984 | |
|---|---|---|---|---|
| | B/A | B/O | B/A | B/O |
| Electric power costs | $ 4,925 | $ 4,925 | $ 6,500 | $ 6,500 |
| All other facility operations costs | 21,347 | 22,680 | 27,000 | 27,000 |
| | $26,272 | $27,605 | $33,500 | $33,500 |
| Beam Hours | 3095 | | 3000 | |

How much additional funding would be required to achieve 3000 beam hours if the Los Alamos-based calculations are used?

If the Los Alamos-based calculations are used, then an additional $2.7 million is required for 3000 beam hours in FY 1984.

Assuming that LAMPF accelerator operations are to be maintained at 3000 beam hours in FY 1984, how do you propose to reconcile the difference between the DOE and Los Alamos calculations?

The differences between the DOE and Los Alamos calculations lie in the escalation indices assumed. Actual beam hours in FY 1984 will depend upon actual costs of facility operations in FY 1984. We will monitor the situation closely as FY 1984 approaches. However, we see little flexibility for reallocation of funds within the Nuclear Physics FY 1984 Budget Request without unacceptable damage to non-LAMPF components of the Nuclear Physics program.

**Department of Energy**
**Washington, D.C. 20585**

March 17, 1983

Honorable F. James Sensenbrenner, Jr.
Ranking Minority Member
Subcommittee on Energy Development
  and Applications
Committee on Science and Technology
House of Representatives
Washington, D.C. 20515

Dear Mr. Sensenbrenner:

Enclosed are responses to questions you submitted to Alvin W. Trivelpiece with your letter dated March 1, 1983. These questions and responses are in connection with the February 24, 1983 hearing before the House Science and Technology Subcommittee on Energy Development and Applications. The subject of the hearing was Authorization for Energy Research Programs (except Fusion).

If you have any questions, please call Ingrid Nelson or Tom Pretorius of my staff on 252-4277. They will be happy to assist you.

Sincerely,

Thomas W. Pretorius
for Robert G. Rabben
Assistant General Counsel
for Legislation

Enclosures

cc: Ms. Christine Wegman
Committee on Science
  and Technology Staff

1. A recent report from the Department of Energy Research Advisory Board strongly emphasized that our national laboratory complex is an impressive asset. How does the budget request take full advantage of this laboratory system?

For FY 1984, the Department plans to allocate over 50% of its total research and development budget to the multi-program laboratory complex. In the annual planning and budgeting process, laboratories submit proposals for new initiatives based on recent developments and discoveries and the laboratory's unique facilities and staff capabilities. Several major initiatives are included in the budget request -- the most important of which for the Office of Energy Research is the National Center for Advanced Materials (NCAM) to be located at Lawrence Berkeley Laboratory. NCAM will be operated as a user facility for U.S. scientists and engineers. It will draw on strengths from the laboratory and the research community in three distinct fields -- materials research, accelerator physics, and instrument design. This is an excellent example of how the laboratory complex can provide sophisticated scientific understanding in areas that are important for future technology and industrial development.

2. This Committee is advised constantly that there is need to stimulate technology transfer to the private sector. Please comment on the validity of such a suggestion and how improved technology transfer can be assured.

The need exists to facilitate technology transfer from our laboratories to the private sector. The potential economic benefits forthcoming to our society make continuing effort worthwhile. We are taking actions to improve technology transfer.

Compliance with the Stevenson-Wydler Technology Innovation Act of 1980 (Public Law 96-480) is an important commitment of DOE. Each laboratory has an Office of Research and Technology Applications (ORTA) or equivalent organization. The ORTA function includes assessment of laboratory technology that may have commercial potential and the support of measures to effect the transfer of capability from the laboratories to industry.

We are searching for innovative and improved methods to render the technology transfer process itself more effective. We are investigating new ideas such as the establishment of institutional arrangements adjunct to the laboratories which could perform technology transfer functions. We are also working with private sector industry associations and venture capital groups to develop better understanding of methods that could enhance industry-laboratory relationships that support technology transfer.

3. With the increase in funding in Basic Energy Sciences, at what capacities do you plan to operate such user facilities as the National Synchrotron Light Source, the Intense Pulsed Neutron Source, and the 1.5 MeV electron microscope?

At the President's budget level we project that each of our user facilities will be operated appropriate to their state of development. In the case of the National Synchrotron Light Source, the budget request will provide for funding of $10.6 million which will enable us to operate this facility at 75% of projected capacity. For the Intense Pulsed Neutron Source, the budget request is $4.5 million and provides for 70% of projected capacity. Finally, the budget request provides $1.8 million for our electron microscope user facilities which we feel will enable full utilization of each of these facilities.

4. Again this year, the request for multiprogram general purpose facilities appears to be extremely small relative to the size of the asset being maintained. Please comment.

The goal of the Multiprogram General Purpose Facilities program is to rehabilitate and replace multiprogram general support facilities that are essential to the continued operation of the laboratories. The program is now in its third year of operation, and although its growth has been modest, it is beginning to show progress toward achieving our goal. This is a long-range program, and we plan to steadily improve the resources allocated to it as we systematically reduce the backlog of deficiencies in these facilities. This effort, of course, must be balanced each year with other demands on DOE resources from equally worthwhile activities.

5. You state in your testimony that an advisory panel composed of scientific leaders from industry, universities, and laboratories will advise the Lawrence Berkeley Laboratory director on National Center for Advanced Materials scientific program directions. To what extent has this advisory board been active in the past and what are future plans for the board's involvement?

We are taking several steps to ensure that the National Center for Advanced Materials (NCAM) program has the advantages of input from the scientific and industrial communities. First, I am proposing to create an ad hoc committee which will report directly to me, that will review the technical and programmatic aspects of this new facility in a way that would involve bringing in individuals from industry, laboratories, and universities. In addition, as I discussed in my testimony, the Director of the Lawrence Berkeley Laboratory will also seek outside participation in the use of the Advanced Light Source and ancillary National Center for Advanced Materials laboratories to assure that the National Center for Advanced Materials basic research addresses America's high technology needs.

An advisory board to the National Center for Advanced Materials, composed of scientific leaders from industry, universities, and Federal laboratories, will advise the Laboratory Director on the Center's scientific program directions. The Advisory Board is in the process of being established and will advise the Director throughout the operation of NCAM. In addition, a National Center for Advanced Materials Industrial Fellows program will be established to provide joint Lawrence Berkeley Laboratory-industry support for participation of industrial scientists in the National Center for Advanced Materials basic research studies of mutual interest to Lawrence Berkeley Laboratory and the scientists' parent companies. Also, the National

Center for Advanced Materials affiliates, consisting of major research teams from industry and universities, will participate in the design, construction, and use of Advanced Light Source beam lines. An Advanced Light Source Program Advisory Committee, comprised of synchrotron light experts from across the nation, will review research proposals for the use of the Advanced Light Source beam lines and rank them according to merit.

6. Please compare the specifications for preparation and maintenance of coal samples at Argonne with specifications at Penn State.

The Argonne and Penn State coal samples have different purposes and very different specifications. The Penn State sample bank is primarily intended to meet the real and legitimate needs of the more applied coal research community; the Argonne sample set is for reference grade samples for the basic research community. The over 1000 coal samples at Penn State were collected and processed in air and then stored under inert conditions. Most of the needs for samples are met from a reserve of cans, each of which holds about one pound of coal sealed under nitrogen. The Argonne samples are to be collected, processed and stored in a carefully controlled atmosphere to insure that samples are completely homogeneous from sample to sample, stable with regard to chemical change over a very long time span and available in sufficient quantity to provide reference grade samples to researchers in the United States and abroad for a long indefinite time period. Samples from the Argonne collection are to be provided in 5 to 20 gram quantities, sealed in glass ampules.

The Argonne collection consists of a very limited number of coals (about 8) in partial response to a 1981 workshop on Coal Sample Banks. Advice and guidance from the entire coal science community was given on the need for additional types of coals and for higher quality reference coal samples than were then available. This was found to be important and necessary because conflicting results have been obtained in the past for measurements on coal samples which were presumed to be the same. Penn State participated in the workshop and its recommendations.

Selection of the specific coals in the Argonne set will be based on recommendations received from the research community and from a User Advisory Committee including representatives from academic, industry and government facilities.

Penn State is now preparing some samples which approach the workshop recommended quality in terms of collection, processing and storage specifications. This set is not intended as reference grade samples, but to provide a broader spectrum of coals which are more chemically durable than the other samples in the Penn State Collection.

7. Concerning the Colliding Beam Accelerator project at the Brookhaven National Laboratory, formerly called the ISABELLE project, would you please set-forth the review process that this project will undergo, including the bodies and scientific panels that will review or are reviewing the project and the dates for their respective recommendations on the project.

Brookhaven National Laboratory (BNL) is preparing a new proposal for a proton-proton Colliding Beam Accelerator (CBA) to build on the existing investment at BNL and their research and development (R&D) results to date. This will provide a candidate for the major physics capability needed by the end of the 1980's, as recommended by the 1981 High Energy Physics Advisory Panel (HEPAP) Subpanel on Long Range Planning. This proposal is expected to be submitted to DOE by March 30, 1983. DOE plans an in-depth technical, cost, schedule and management review of the BNL proposal April 11-15, 1983.

A 1983 HEPAP New Facilities Subpanel has been formed which will be chaired by Professor Stan Wojcicki, Chairman of the Department of Physics, Stanford University. The Subpanel is charged by DOE to consider and make recommendations on the scientific requirements and opportunities for a forefront U.S. High Energy Physics program in the next five to ten years, to make specific recommendations for any FY 1985 new construction items and to give a definite recommendation regarding the BNL CBA. The Subpanel had its organizational meeting on February 25 and 26. The Subpanel will make on-site visits to BNL, Fermi National Accelerator Laboratory and Stanford Linear Accelerator Center during April and May each of which will include discussions with the university/user community. The Subpanel will conclude its work with a week long session at Woods Hole, June 5-11, 1983. The report resulting from the Subpanel's deliberations will be reviewed by HEPAP at its meeting on June 29-30, 1983. HEPAP will then forward the report to DOE with their comments and recommendations.

8. Concerning the Colliding Beam Accelerator project, would you please indicate the review process by the Department of Energy of the reports and recommendations of the bodies and scientific panels that are studying the project and the timetable for any recommendations by the Department of Energy.

As I indicated in answer to your previous question, the review process is a complex one involving reviews and advice from the High Energy Physics Advisory Panel and its 1983 Subpanel on New Facilities and separate reviews by DOE with consultants where appropriate. The final decision rests with DOE and will take into consideration all of these reviews and recommendations and will be made in July 1983. The Department's decision will be reflected in its FY 1985 budget request to OMB.

9. When does the Department of Energy expect to make a final decision concerning the Colliding Beam Accelerator facility?

DOE plans to make its final decision concerning the Colliding Beam Accelerator facility in July 1983.

10. Prior to the start of FY 1985, how does the Department anticipate commencing construction of the facility?

The Department has no plans to commence construction of this facility prior to the start of FY 1985.

11. Will the Department of Energy request a supplemental appropriation for FY 1984 if the facility is recommended to be constructed?

Because of the research and development efforts required in FY 1984 to adequately test the magnet design, prototypes and systems for the Colliding Beam Accelerator facility, the Department does not envision requiring such a supplemental appropriation.

Dr. KROPSCHOT. Mr. Chairman, it is a pleasure to appear before you today to discuss the fiscal year 1984 budget request for basic energy sciences. As a brief background, the mission of the DOE basic energy sciences, or BES, as I will call it throughout my presentation, is to conduct the basic research in support of the Nation's energy options for the future. The fields of energy that we cover are the fields of physical sciences, those of physics, chemistry, mathematics, materials science, geology, as well as engineering and the biological sciences. BES develops the longer term technology base relevant to all energy technologies, and the important key point is that our major products are the knowledge and understanding of physical sciences.

It may be true that the United States is less dependent on foreign energy sources today than we were 10 years ago, but there is a continued need for us to upgrade our technology base, both in existing technologies like fossil, for example, and equally important for us to establish a base in new technologies, like innovative new technologies for solar. It is this area, or the technologies which will emerge in the next few decades that are the resources we have for energy. It is such research which will result in new options for the future.

BES supports over 1,200 different projects conducted by university, industrial and national laboratory scientists alike, and in addition, our research, like all physical research, is increasingly dependent on large, thus expensive, many times unique facilities. I would like to highlight some of our progress that has taken place in the last few years. These facilities include nuclear research reactors, nuclear accelerators which provide synchrotron radiation, combustion research facility, and several electron microscopes.

The National Synchrotron Light Source at Brookhaven National Laboratory was dedicated just this last November, November 1982, and an active research program is already underway. The Intense Pulse Neutron Source at the Argonne National Laboratory and a 1.5 million electron volt, electron microscope at Lawrence Berkeley Laboratory, have just completed their first full year of research operation.

I am also pleased to announce that the atomic resolution microscope, the $7 million construction project that this committee was very influential in approving in the fiscal years 1980 and 1981 has been delivered. It is in place, and the last bit of wiring is being installed. The final acceptance tests will take place in the next month so that will add another dimension to our Center for Electronic Microscopy at Berkeley, along with the 1.5 million electron volt microscope, and the related activities in research in the smaller microscopes there.

It is difficult for me to convey to you the enormity of the basic energy sciences program in terms of accomplishments. Each year we pull together a series of summaries of these accomplishments. We previously provided copies of these accomplishments to your staff, and I am sure copies are available from them. We pull together that kind of information every year.

These accomplishments illustrate 36 projects that have come to fruition as a result of research that has gone on in the past, and we try to illustrate through those types of examples what BES is all about.

For example, in our materials science program, we have identified the causes of stress corrosion cracking in nickel alloy materials, and developed a control mechanism for this cracking. The stress corrosion cracking is a well documented cause of costly and unscheduled shutdowns of nuclear powerplants.

In the chemical sciences program we have developed a new scheme which is more precise by thousands of times, maybe tens of thousands of times, in its ability to measure sulphur. To be more specific, each of the elements in the periodic table have isotopes, and what we are talking about here is our ability to measure the ratios of isotopic sulphur, and the precision with which we can do it. That is important, because it turns out the origin of the sulphur is dependent on whether it come from coal, from fossil burning, or from volcanoes or from agricultural products, and it may be a scheme that can be used to sense where the sulphur originates in the atmosphere.

I could go on with discussion of the accomplishments, but I would like to stop at this point and go on to a little bit more detail on the nature of our budget, and discuss with you what we are trying to accomplish in BES, and explain a bit about the background. The total BES request for fiscal year 1984 is $350.2 million. The increase over fiscal year 1983 is requested for three different things.

First of all, we want to support a strong core program in materials science, chemical science, nuclear science, engineering, mathematics and geosciences, the advanced energy projects, and our biological energy research.

Second, we want to expand and more effectively use our unique facilities.

Third, and most important for this discussion, as Dr. Trivelpiece mentioned earlier, to exploit new research opportunities, and in particular our initiative on materials.

Rather than give you great detail behind this request, let me explain what we are trying to accomplish. I stated earlier that BES, through research, is a source of knowledge and understanding. Now we live in a complex technological society, and we have no end of problems. Our job, BES's job, is to characterize the important ones of these problems, and sort them out in relation to our mission—after all, we are a mission agency—and then to provide solutions through proper research.

Specifically materials plays a pivotal role in every energy technology, and much of our international competitiveness comes because of our knowledge and our ability to use advance materials. Microcircuitry, aircraft turbines, and aircraft are two cases in point. Our competitiveness comes because of this advantage.

Also we are experiencing extensive losses due to materials failures, the $80 billion for corrosion that Dr. Trivelpiece mentioned, and we have also identified in a recent study $120 billion additional because of premature fracture. Large leverage exists in research. DOE alone expends over $300 million annually in R&D.

The total Federal expenditure is about $1 billion. Still, there are many reports and studies conducted by scientific leaders of the Nation, that tell us more research is needed and that clearly indicate that materials research is a top priority.

Thus, we are proposing a significant increase in our budget, and we have requested two construction projects, one for the National Synchrotron Light Source at Brookhaven National Laboratory and the National Center for Advanced Materials at the Lawrence Berkeley Laboratory.

The National Synchrotron Light Source will provide the needed experimental laboratories adjacent to the machine and fully develop eight new beam lines.

The Center for Advanced Materials has several parts. We are asking for a new synchrotron light source, which has a brighter, a more intense beam, and different pulse characteristics which will allow us to do new and different experiments heretofore not possible. The justification for the new machine is based on the premise that synchrotron light has already revolutionized whole new areas of science, opening up entirely new fields.

Then building upon the BES competence at the Lawrence Berkeley Laboratory and the University of California, senior scientists and the research capabilities of the graduate students involved, we are proposing a surface science and catalysis building. Many times properties are dominated by what happens with the surface, like the corrosion that was mentioned.

Then there is a second building, an Advanced Materials Synthesis Laboratory and an Advanced Device Concepts Laboratory. Synthesis of materials is a rapidly developing new science where by understanding how and controlling how atoms go together on a microscopic scale we can make entirely new materials heretofore not available.

This is the thrust of our center's activity that we are trying to pull together, to take the information that has already been provided on what is needed in materials science, and address that issue.

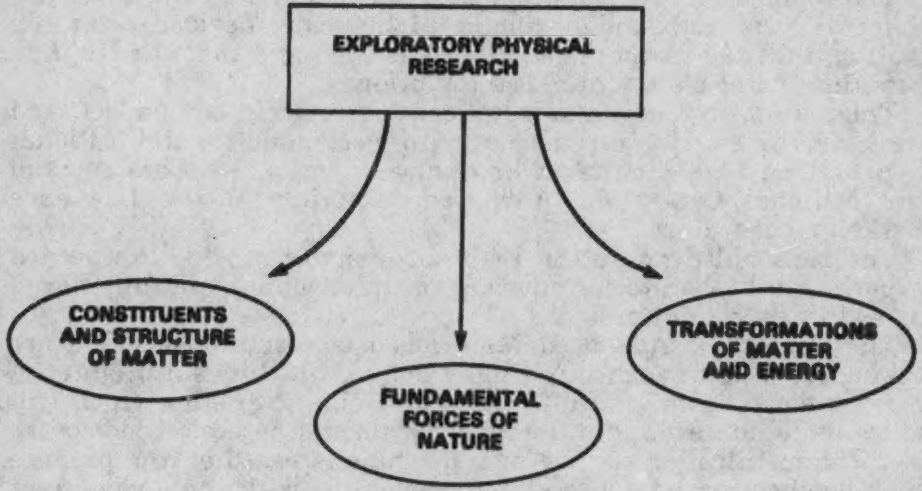
BES already has a large involvement at Lawrence Berkeley Laboratory and at Brookhaven, with the synchrotron light source and the reactor, and this request can take advantage of those resources, to build upon the competence base to conduct a new and exciting and important research area in the future.

Thank you, Mr. Chairman. That is the end of my testimony. I would like to turn it over now to Dr. Leiss.

Dr. Leiss. Thank you, Mr. Chairman. It is a pleasure to speak to you again.

I would like in a few minutes to provide an overview of the high energy and nuclear physics program, and what in a sense in the first few pictures is something of the idealism of the field; and then turn specifically to the program.

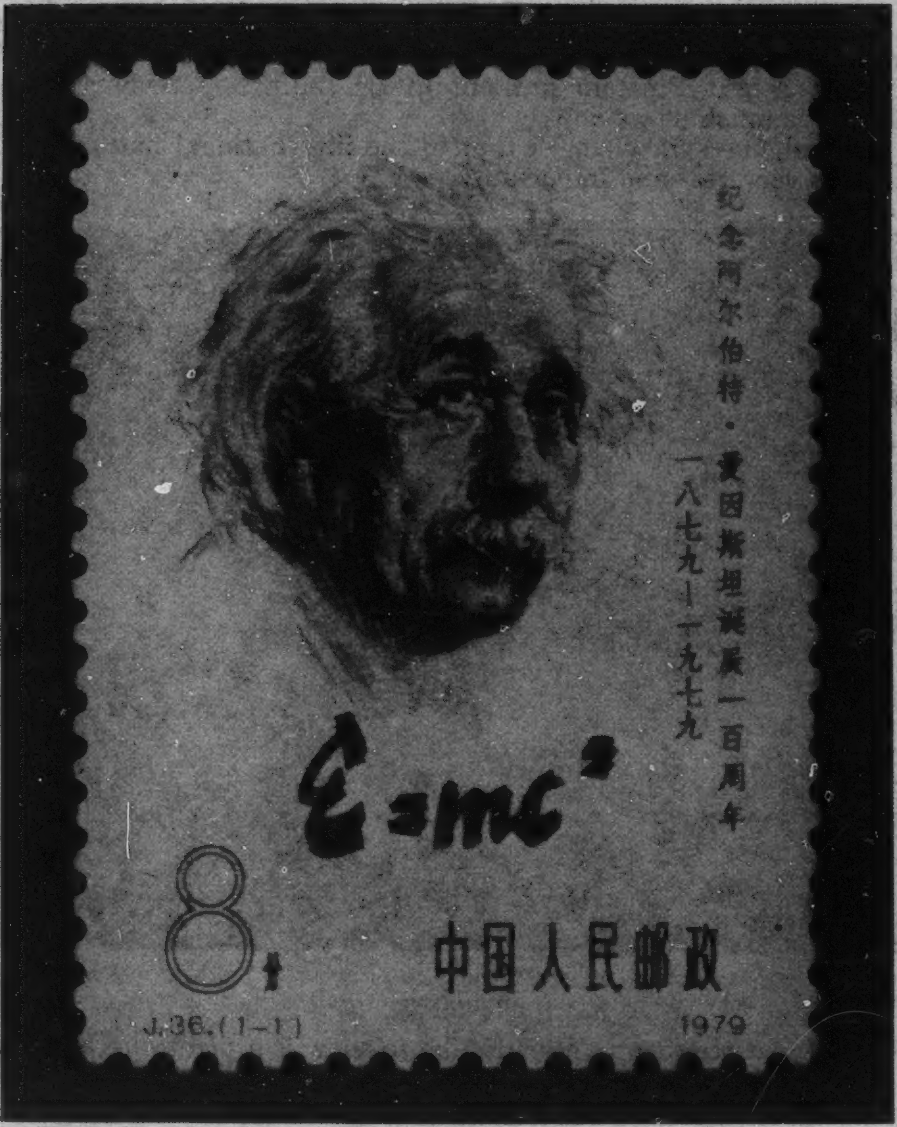
## HIGH ENERGY AND NUCLEAR PHYSICS


EXPLORATORY PHYSICAL RESEARCH
CONSTITUENTS AND STRUCTURE OF MATTER
FUNDAMENTAL FORCES OF NATURE
TRANSFORMATIONS OF MATTER AND ENERGY

VIEWGRAPH 1

High energy and nuclear physics, is an area of exploratory physical research. We try to understand the constituents of matter, the fundamental forces of nature, and the laws that govern them.

This is an area of research that has attracted and intrigued man almost throughout our recorded history. It is the laws of physics and understanding of nature at the most fundamental level which drives and provides the basis for all of the science and technology that we live with.


纪念阿尔伯特·爱因斯坦诞辰一百周年
一八七九—一九七九
E=mc²
8分
中国人民邮政
J.36.(1-1)
1979

VIEWGRAPH 2

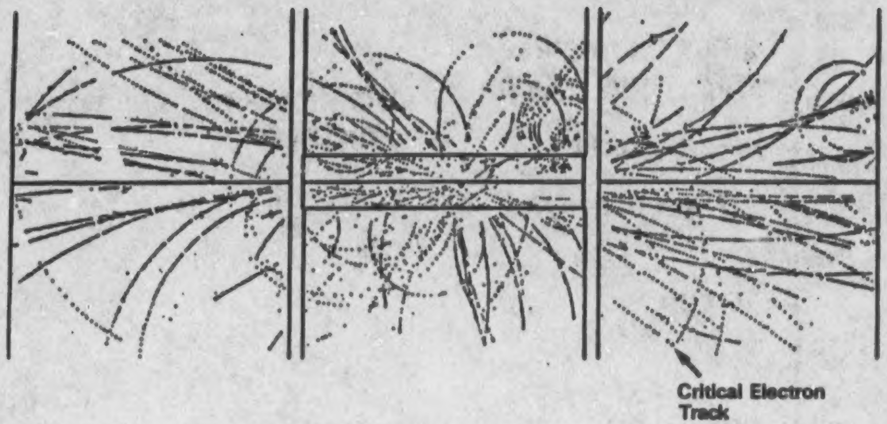
It is truly international and to symbolize that I would like to show you a picture that I found in Beijing, China. It was very fascinating to me, but here is a stamp commemorating the 100th birthday of Albert Einstein put out in Beijing. They revere him in China as much as we do.

I think the symbolism of that in the international nature of science is very real and important.

VIEWGRAPH 3

The next viewgraph I would like to show is a picture of a galaxy. Mankind has really tried to understand and be intrigued by stars. This represents the very large.

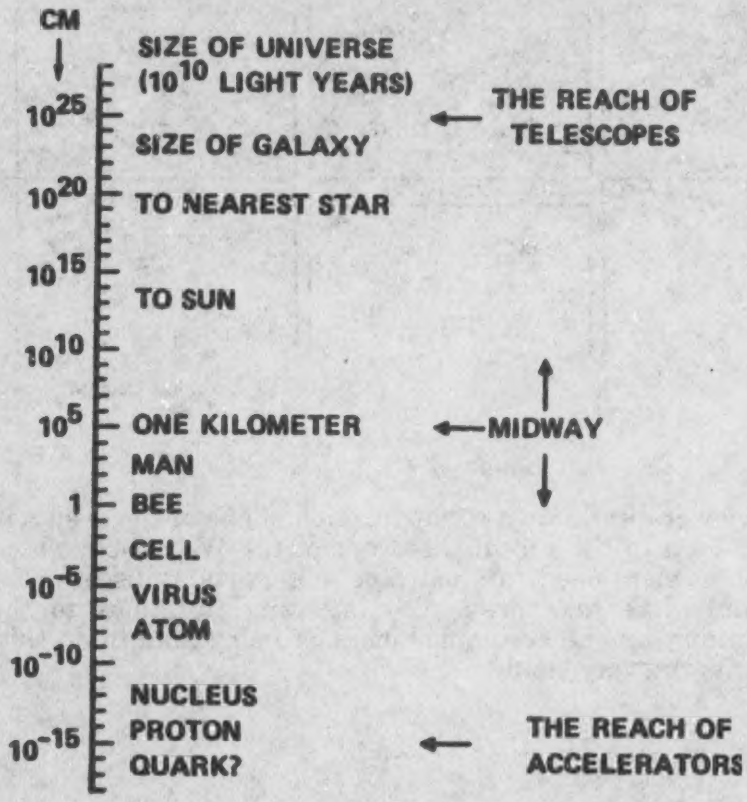
## RECONSTRUCTION OF W PARTICLE EVENT



Viewgraph 4

The next viewgraph shows a reconstruction of one of the events in the detector, used in the recent discovery of the W particle which Dr. Trivelpiece mentioned, an extremely important discovery. I would certainly like to express also my congratulations to the CERN Laboratory on the accomplishment of being able to do this. This represents the very small.

## THE SCALE OF NATURE

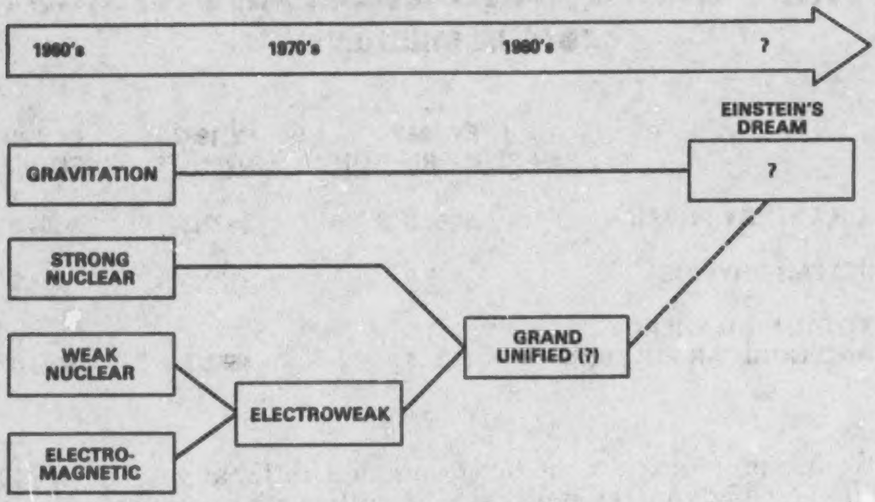


VIEWGRAPH 5

The next viewgraph shows this on a scale, the very large, enormous distances, and the very small enormously small distances. This is where the frontiers of trying to understand the basic laws of nature have led us.

Another intriguing thing about that is the tremendous synergism that now exists between research in astrophysics and understanding the universe and the big bang, and research in high energy physics. They are feeding each other. It is beautiful to behold.

## HIGH ENERGY PHYSICS
## UNIFICATION OF FORCES



VIEWGRAPH 6

In the next viewgraph, I would like to try to attempt to explain the importance of the discovery of the W particle. This shows a picture, that I believe I showed you last year, of the four known forces of nature, gravitation, the strong nuclear force, the weak nuclear force, and the electromagnetic force. It was a dream of Einstein that all of these forces were derivatives of one common force and one force law; that if we were smart enough, we could understand everything in terms of one force.

The theories that predicted the unification of the weak nuclear force and the electromagnetic force into what we call the electroweak force have been in the past partially confirmed by specific detailed measurements which made that theory sufficiently credible that the Nobel prize was given to the theoreticians for it. But the really important thing that would make the theory stand was, can you find those W and Z-zero particles that were predicted by that theory and were they where they were supposed to be?

Now, the W particle found at CERN is exactly where the theory said it would be based on the existing experimental evidence, and is produced at the rate expected, although only a very few events have been seen.

The challenge, the future challenge in high energy physics, will be to get sufficiently strong sources of these particles in machines such as the Stanford linear collider that I will speak to in a minute, so that we can do detailed studies of the detailed predictions of that theory, looking for discrepancies between the experiments and the theory, so that one can get clues to push on further toward the grand unified theory, which is the next step. In a sense

I have tried to put the whole thrust of the present high-energy physics program into this chart.

## HIGH ENERGY AND NUCLEAR PHYSICS

**(B/A IN MILLIONS)**

| | FY 1982 APPROPRIATION | FY 1983 ESTIMATE | FY 1984 REQUEST |
|---|---|---|---|
| HIGH ENERGY PHYSICS | $364.5 | $422.2 | $490.0 |
| NUCLEAR PHYSICS | 133.9 | 130.5 | 153.0 |
| TOTAL HIGH ENERGY AND NUCLEAR PHYSICS | $498.4 | $552.7 | $643.0 |

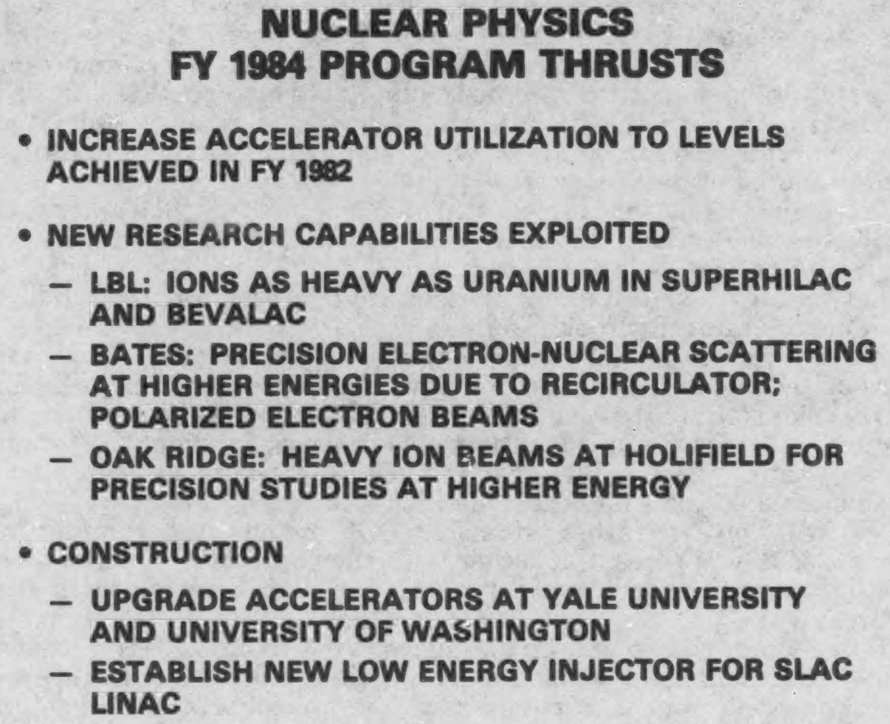
VIEWGRAPH 7

We are proposing for the budget request in fiscal year 1984, $490 million for high energy physics, $153 million for nuclear physics, or a total of $643 million.

## HIGH ENERGY PHYSICS
## FY 1984 PROGRAM THRUSTS

- CONTINUED GOOD OVERALL UTILIZATION OF FACILITIES (55 – 60%)
  - INCLUDES FIRST FULL YEAR OF OPERATION OF ENERGY SAVER
  - STRONG SUPPORT FOR UNIVERSITY GROUPS
- CONTINUED PROGRESS ON TEVATRON-I AND TEVATRON-II
  - CONSTRUCTION AS PLANNED
  - STRONG EQUIPMENT SUPPORT FOR SECONDARY BEAM LINES AND DETECTORS
- INITIATE CONSTRUCTION OF SLC ON FAST SCHEDULE – CONSTRUCTION COMPLETION FY 1986
- CBA PROGRAM AT BNL PROJECTED AT FY 1983 DOLLAR LEVEL
  - R&D ONLY – NO CONSTRUCTION REQUEST
  - NATURE, SCOPE, AND FUNDING IN FY 1984 DEPENDENT ON JULY DECISION ON NEW PROPOSAL

VIEWGRAPH 8



## NUCLEAR PHYSICS FY 1984 PROGRAM THRUSTS

- INCREASE ACCELERATOR UTILIZATION TO LEVELS ACHIEVED IN FY 1982
- NEW RESEARCH CAPABILITIES EXPLOITED
  - LBL: IONS AS HEAVY AS URANIUM IN SUPERHILAC AND BEVALAC
  - BATES: PRECISION ELECTRON-NUCLEAR SCATTERING AT HIGHER ENERGIES DUE TO RECIRCULATOR; POLARIZED ELECTRON BEAMS
  - OAK RIDGE: HEAVY ION BEAMS AT HOLIFIELD FOR PRECISION STUDIES AT HIGHER ENERGY
- CONSTRUCTION
  - UPGRADE ACCELERATORS AT YALE UNIVERSITY AND UNIVERSITY OF WASHINGTON
  - ESTABLISH NEW LOW ENERGY INJECTOR FOR SLAC LINAC

VIEWGRAPH 9

The next viewgraph summarizes the thrust in the high energy physics program that we would carry out with this budget.

In fiscal year 1983, we made a very deliberate attempt to increase the utilization of our high energy physics facilities, which had fallen to a fairly low level because of inflation, increased power bills and so forth, and we felt it was necessary to increase that utilization.

We would continue that 55- to 60-percent utilization in fiscal year 1984 and would have our first year of operation of the superconducting magnet synchotron at Fermilab that will come on the air toward the end of this fiscal year.

We will also continue to provide stronger support to the university groups that we have started in fiscal year 1983.

We would continue as planned the construction of the Tevatron I and Tevatron II facilities at Fermilab. These will allow us for both colliding beams and fixed target experiments the highest energy beams available in the world.

We will initiate construction of the Stanford Linear Collider on a relatively fast schedule. There has been intensive R&D on this facility going on for several years. We believe the projects are ready. This will allow us really to accomplish two things:

First, it is a relatively inexpensive add-on. It is a lot of money, but relatively to say the cost of building the large electron-position [LEP] facility at CERN, which is close to $1 billion. We can try out a new concept on how to build electron-positron colliders in the future.

If this is sucessful, as we believe it will be, we will then be able also to exploit this linear collider to do physics with the W- and Z-zero particles in an energy region where there is very important research to be done with a copious supply of these particles. I tried to describe earlier the importance of having a copious supply so you can really look in detail at what is going on to guide the future undestanding of nature.

We believe that this project can be completed about the end of 1986, and would then help maintain the U.S. leadership position in high energy physics in the world.

Last, we will continue our planned R&D program on a colliding beam accelerator at Brookhaven.

As you know, the Isabelle project at Brookhaven ran into rather serious technical difficulties. We have supported an R&D program at Brookhaven to resolve their difficulties, to look at ways to reduce cost, to come up with their new proposal for what they want to do. That proposal will be made to us in March of this year, along with estimated costs and their justification.

We will have a rather intensive DOE review and community review of their proposal, together with the community's perception of the scientific priorities that we should obey and follow in the future.

Based upon the community's advice, we will make our decision and will recommend what should be the future of a facility at Brookhaven. To put it in perspective, the project was started sometime ago. It is clear that it will be completed considerably later than was originally planned.

I think it is proper, for a project of that expense, that we take a very hard look at what is the right thing to do in terms of priorities and opportunities for important research. We will be conducting that review this summer, together with a review of how Brookhaven has done on their R&D efforts.

I turn now to the nuclear physics program.

As I mentioned, in 1983 we made a very intense effort to increase utilization of our high-energy physics facilities. In fiscal year 1984 we are trying to do the same thing, increase utilization on our nuclear physics facilities, which, as I believe you know, were underutilized, particularly at the 1983 budget level. Part of the increase in the nuclear physics program is to improve this utilization picture.

In research activities, we will exploit three new capabilities that we have in the nuclear physics program, and of which we are very proud.

At the Berkeley Laboratory, we can now accelerate ions as heavy as uranium to over 1 billion electron volts per nucleon, and start understanding what happens when high-energy heavy ions collide with each other.

We have completed the upgrade for recirculation of electrons through the Bates linear accelerator. For a relatively small sum of money we have doubled the energy of that machine and allowed important new research.

Last, we have full use of the Oak Ridge Holifield heavy ion accelerator facility.

In construction activities, as Dr. Trivelpiece mentioned, we are proposing a construction project to upgrade the accelerators at the

University of Washington and Yale University. These are two major educational institutions training nuclear scientists in America.

Finally, we would add a new injector, a large fraction of the way down the accelerator, on the Stanford linear accelerator, to make that machine attractive to a certain subset of experiments that nuclear physicists are extremely anxious to perform.

Mr. Chairman, that completes my comments.

I turn it back to Dr. Trivelpiece.

Dr. **Trivelpiece**. Mr. Chairman, I know you have many questions. I had an optional slide show prepared for you, but I believe we have run over our time a little bit. I thought perhaps that some of the newer members would like to have a brief picture show of facilities.

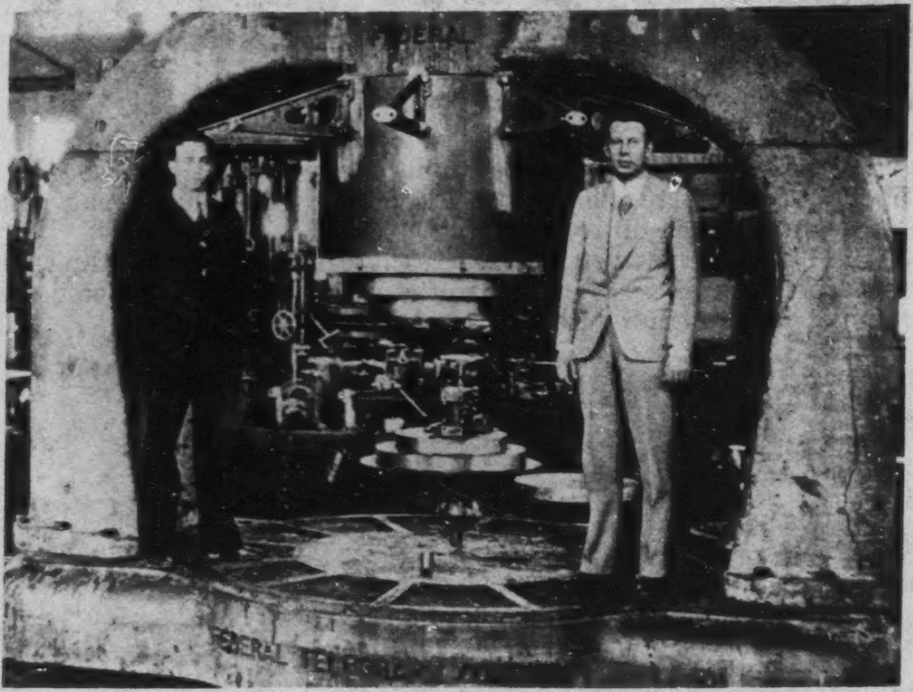
I would be glad to do it or I could arrange to do it later.

Mr. **Fuqua**. How long would it take?

Dr. **Trivelpiece**. I would estimate that it will take less than 10 minutes.

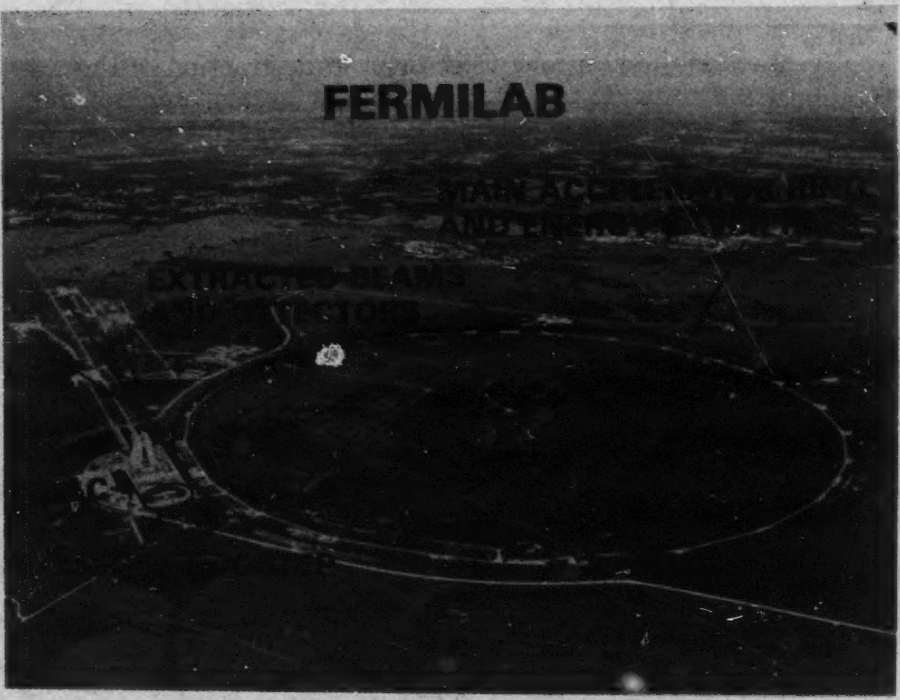
Mr. **Fuqua**. Why don't we do that then? I hope I can get out to see them in person sometime.

Dr. **Trivelpiece**. I would like to arrange that at any time you can do it.



Viewgraph 10

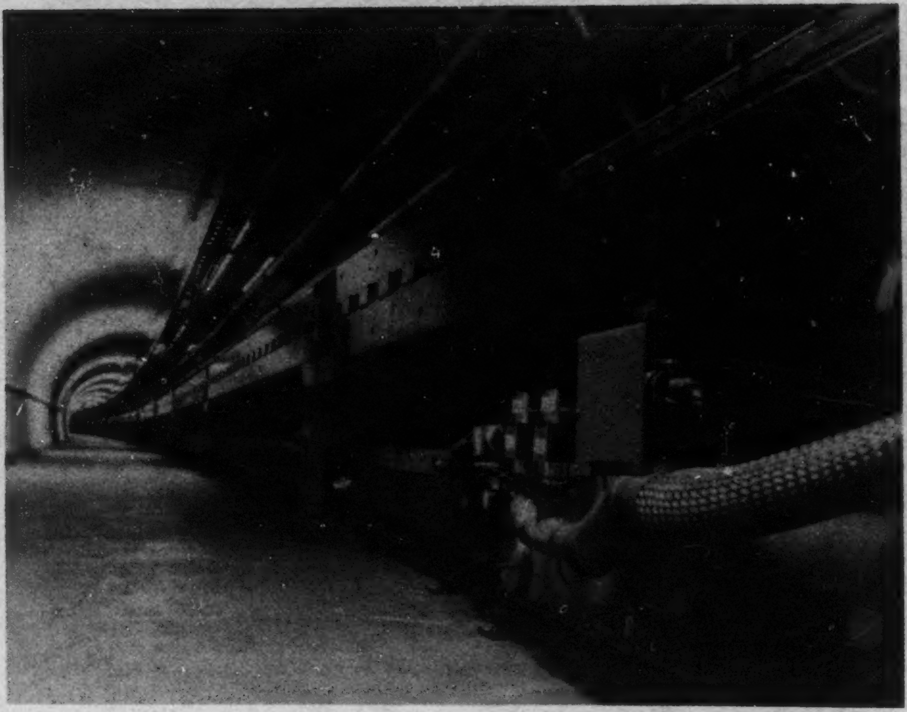
This is a picture in the thirties showing Dr. E. L. Lawrence standing in the yoke of a magnet that was the first accelerator, the first cyclotron; it was one of the early cyclotrons at Lawrence Berkeley Laboratory. It gives you some idea of the size of things as they existed in the thirties.


FERMILAB
EXTRACTED BEAMS
AND DETECTORS

Viewgraph 11

The next slide shows the Fermilab. The ring that you see in the middle of that picture is about 6.2 kilometers in circumference, and the beam lines are off to the left.

The building that you can see is the central office building and the experimental areas are off to the upper left. That is some estimate of the growth in size of these facilities.



VIEWGRAPH 12

What is inside that tunnel are strings of magnets. The magnets are used to guide the particles as they go around their circular orbits and are accelerated.

The upper magnets are the original ones constructed; the ones below are the superconducting magnets added later. They are the so-called saver magnets that will permit utilizing the same facility up to much higher energy-to one TeV on a fixed target and eventually one TeV particle colliding with each other to give two total TeV interaction of energy.

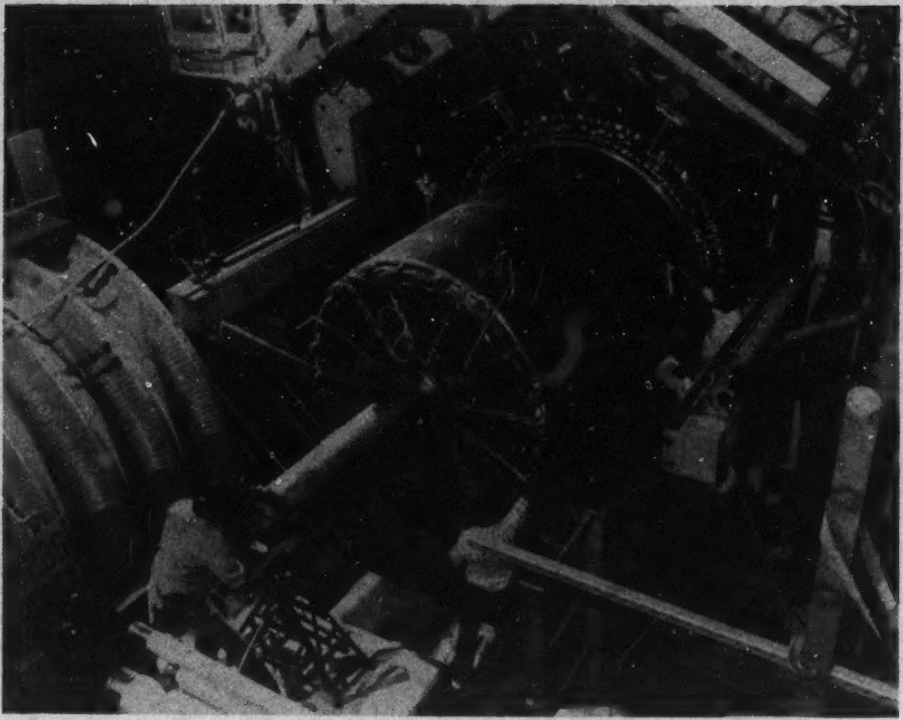
VIEWGRAPH 13

The rather modest kind of detector that is needed in order to explore these rather small bits of matter is illustrated here. There isn't a human being standing in the picture that I can see, but the chair gives you some idea of the size of those detectors. The large discs of iron you see are used to cause one of the more elusive of fundamental particles of matter, the neutrino, to have adequate mass to interact with and then produce evidence of its existence and cause other means by which it can be measured to be seen.

VIEWGRAPH 14

The Stanford Linear Accelerator Center and the proton-electron project is shown here. It is a two-mile long linear accelerator which causes electrons and positrons to be put into that circular ring in counterrotating directions.

The new Stanford Linear Collider will make contact with that ring at the point where the straight section meets the circular part, but it will go in a circle which is much larger than can be shown on that particular viewgraph.



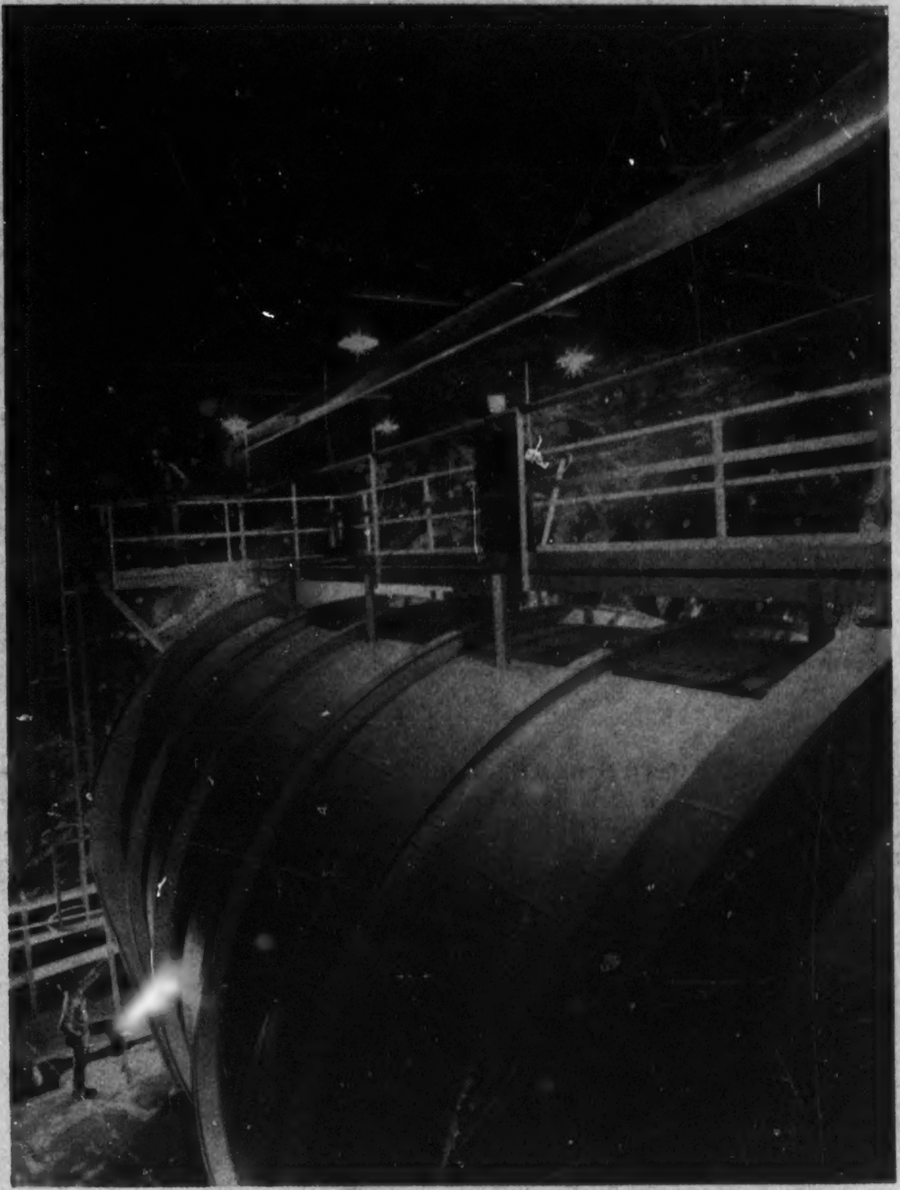
Viewgraph 15

The kinds of detectors that are needed in order to capture the rather elusive events that take place when a positron and an electron annihilate and create new matter out of energy are shown here. The results of the interactions are then captured in this device which automatically records the character of the event, sends it directly to a computer, compares it with theoretical analysis and allows the scientist to more or less, in real time, explore some of the outcome of these events.

VIEWGRAPH 16

This is the Alternating Gradient Synchroton at the Brookhaven National Laboratory. It is currently in operation. Those then, are the three major U.S. High Energy Physics Centers. Stanford, Fermilab and Brookhaven are the major centers for high energy physics research in the United States.

The next viewgraph.



Viewgraph 17

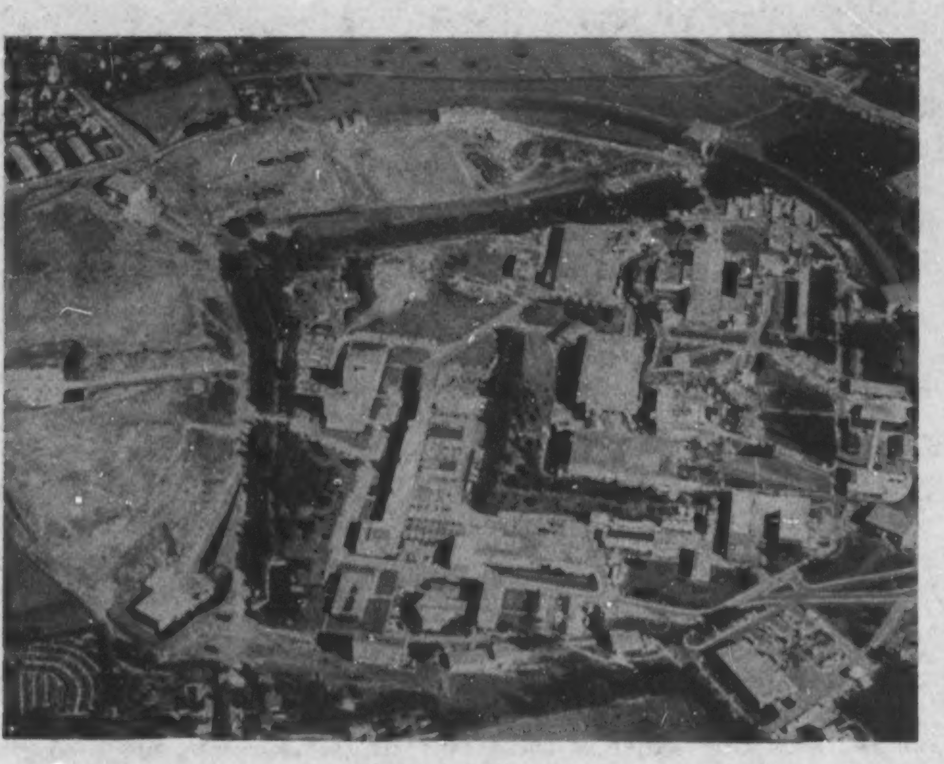
Occasionally things other than accelerators, are used. In this case, this is a tank of liquid that is down at the bottom of a mine. It is used to explore whether or not protons actually live for 10 to the 31st years or not. Protons are thought to decay. It is one of the predictions of theory.

Scientists are currently looking at whether or not protons decay at a rate that is consistent with that theory. It takes this kind of a facility in order to explore that question.

VIEWGRAPH 18

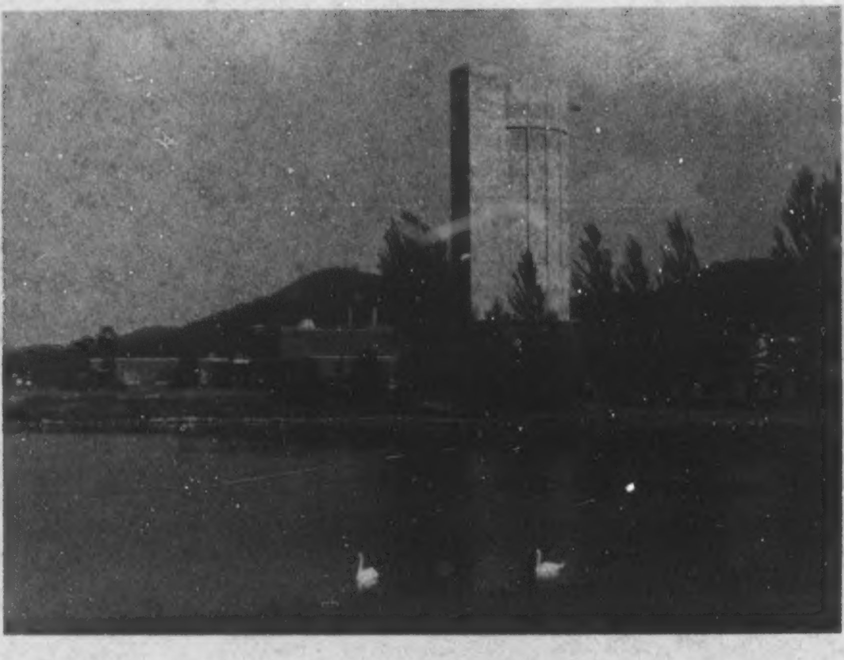
Next, this is a picture of a map of the area near Geneva in Switzerland. What is at the center, the small dark circle, is the present accelerator ring which is the facility that discovered the W particle—the CERN Super Proton Synchrotron project at Geneva.

The larger dotted circle is 17 miles in circumference. That is the tunnel that is under construction already. It will house the so-called large electron-positron project lab that will give the Europeans a considerable advantage sometime in the future.



VIEWGRAPH 19

This is another European facility. It is at Hamburg. It is the so-called DESY facility. They are planning to upgrade this facility in order to permit them to have electrons collide with protons at very high energies.



VIEWGRAPH 20

Turning now to nuclear physics, this is the Holifield heavy ion research facility at the Oak Ridge National Laboratory.

What is contained in the building is, an accelerator for accelerating positive particles at intermediate energies.

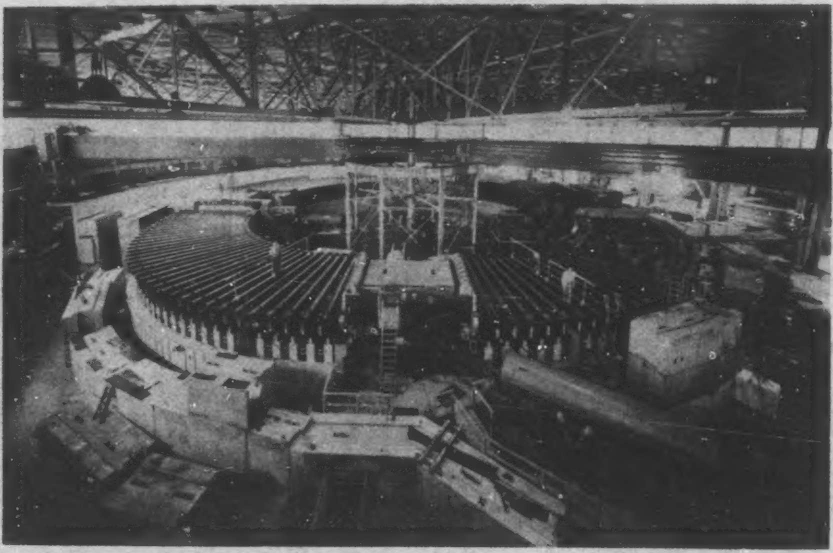
VIEWGRAPH 21

Dr. Leiss referred to the Bates accelerator at Massachusetts Institute of Technology. This is it, to give you some idea of the size of the facility.

One of the things that is done at the Bates accelerator is to have electrons accelerated to high speeds before they strike targets in order to produce other subatomic particles, and then to use the subatomic particles for further studies.

VIEWGRAPH 22

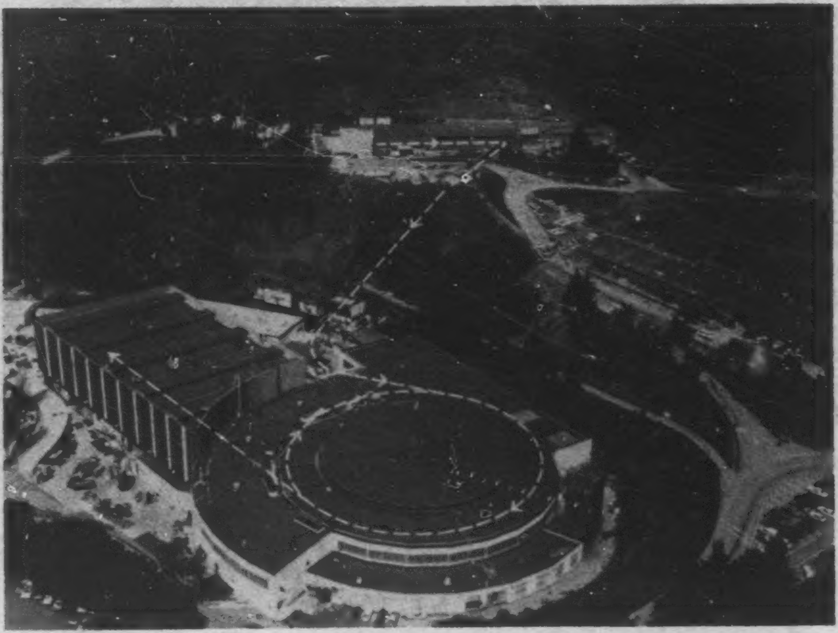
This is the so-called Los Alamos meson facility. It is located at the Los Alamos National Laboratory.



VIEWGRAPH 23

The Bevatron is the facility which is now being used to accelerate fully stripped uranium atoms. It is at the Lawrence Berkeley Laboratory.

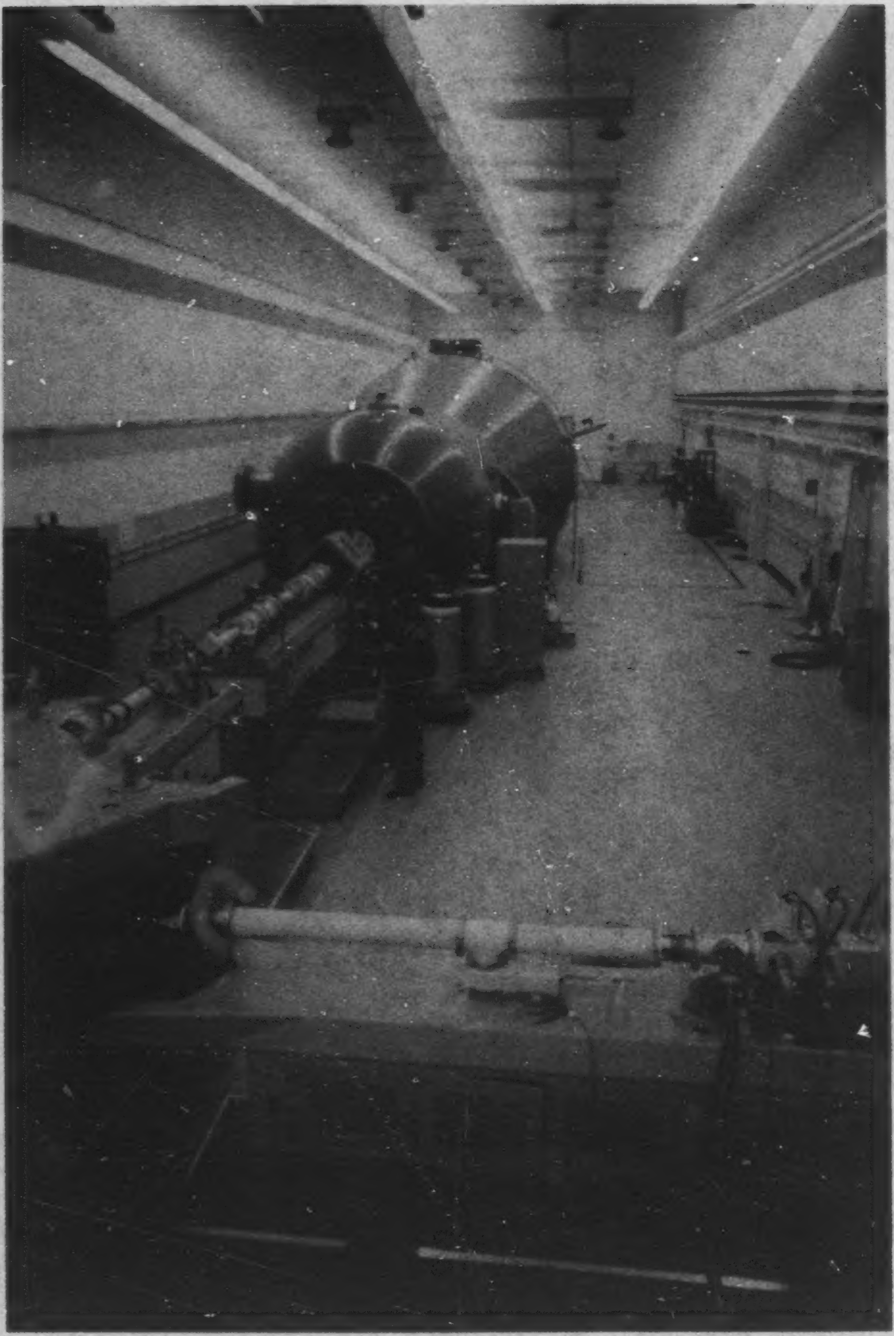
This was originally created for other purposes, but is now being used to accelerate uranium atoms.



VIEWGRAPH 24

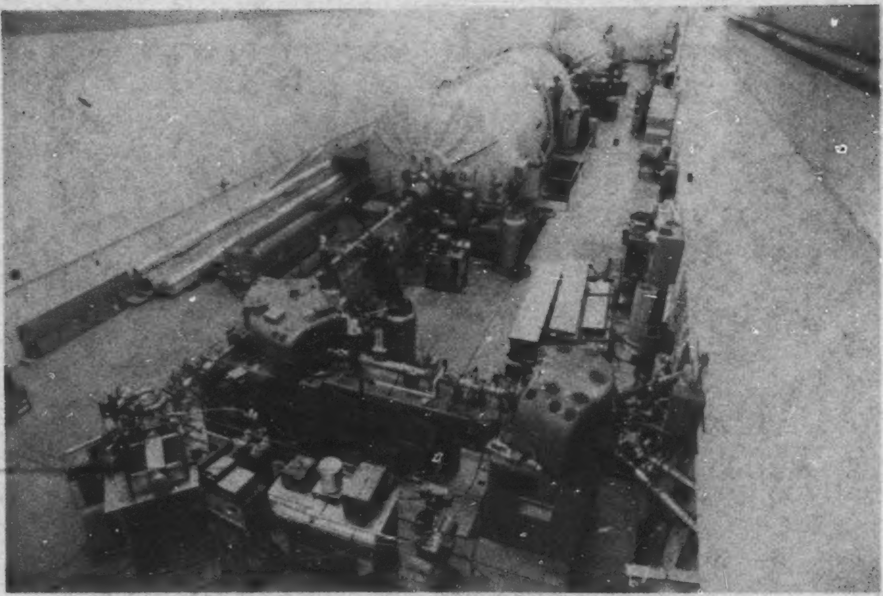
This gives you some idea of how inventive physicists are in this area. The building that contains the Bevatron is shown in the lower portion of this picture. The dotted line that goes across the valley is a tube that connects the facility that is in the upper portion of the picture which is called the HILAC. It is a linear accelerator. It begins the particles on their journey to high speed, and they are shipped over to the Bevatron and the Bevatron then accelerates them to higher energy.

The combination of these two facilities is a compound word, Beva-lac.



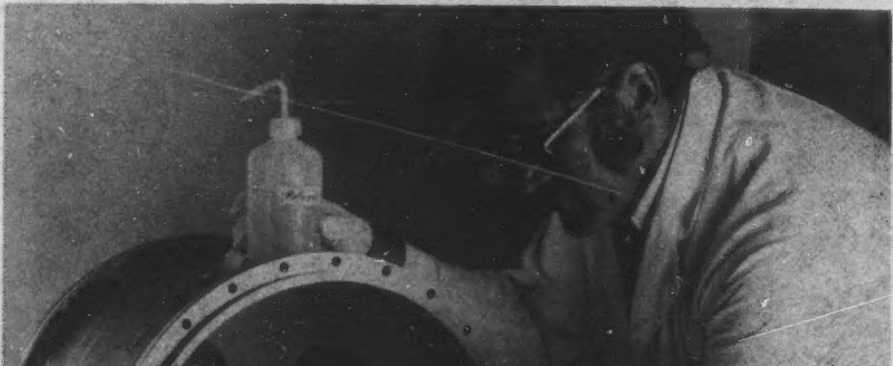
Viewgraph 25

This is the accelerator at Yale University. It will be upgraded to higher energy as a result of the initiative we are proposing.



VIEWGRAPH 26

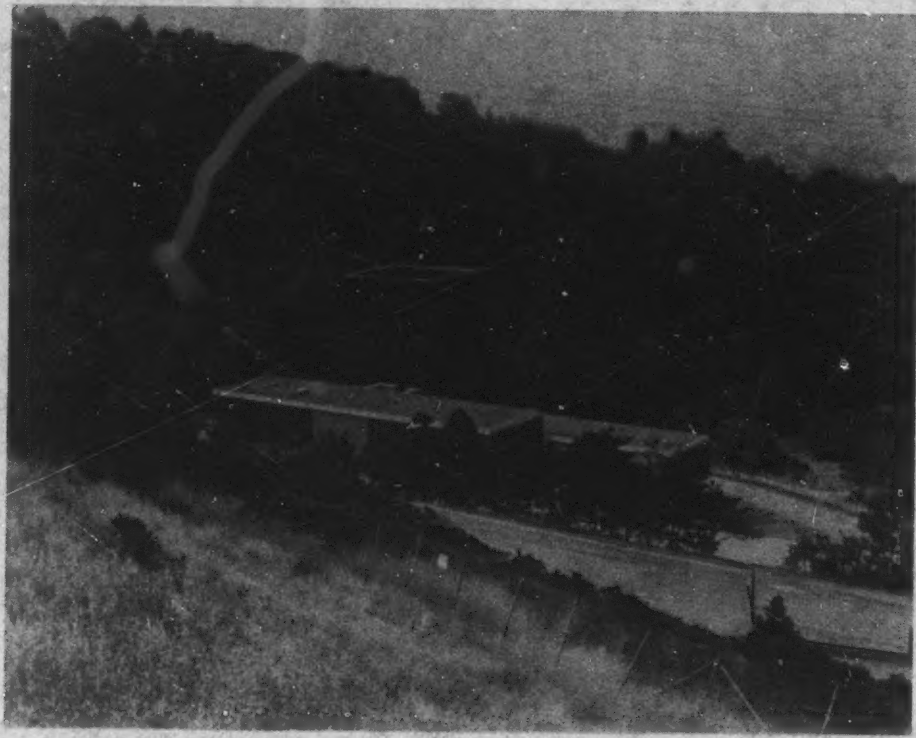
This is the facility at the University of Washington. It will be upgraded in not quite the same manner. It will involve some other apparatus which I will show you in the next viewgraph.



VIEWGRAPH 27

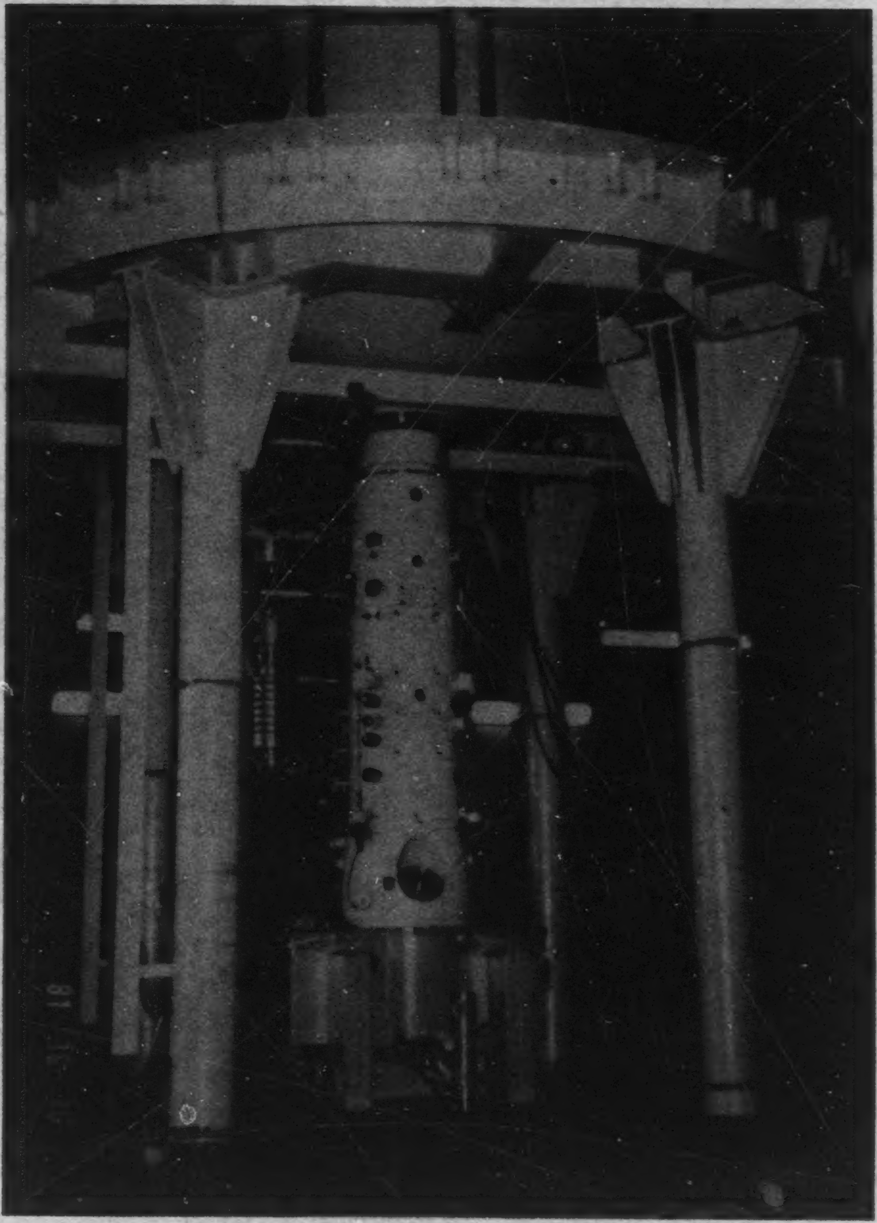
You have all heard of superconductivity. Superconductivity permits currents to flow in conductors without any appreciable losses. By coating the inside of this particular object that you see here with a material that can be made superconducting, particles can be accelerated to high energy with much less electrical energy investment than was possible in the past.

This particular device that you see here is used on an accelerator at the Argonne National Laboratory. However, the same technology will be added to the University of Washington's facility in order to give it higher energy and make this world class facility available to students for research.



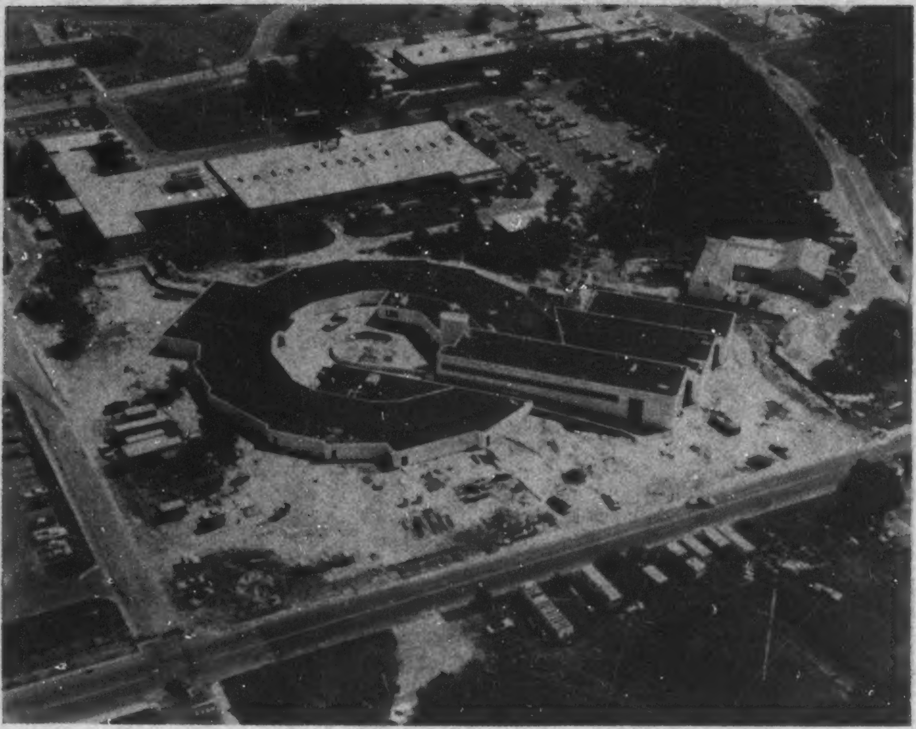
VIEWGRAPH 28

This is a building that houses one of the microscopes at the Lawrence Berkeley Laboratory. It is one of the facilities that will be available along with the National Center for Advanced Materials we are proposing in fiscal year 1984.



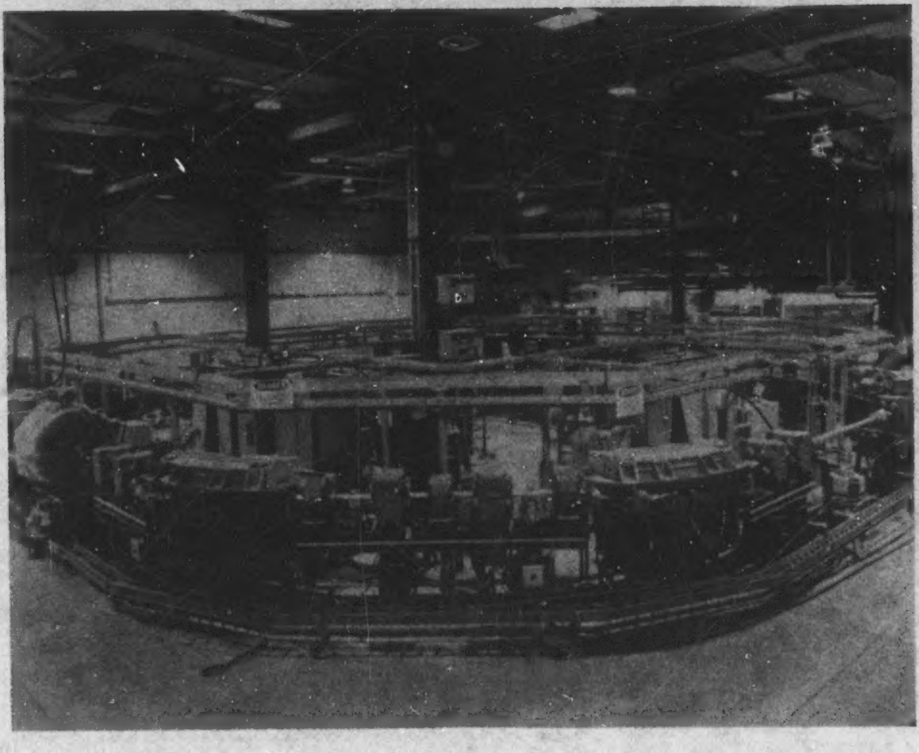
VIEWGRAPH 29

Dr. Kropschot referred to the atomic resolution microscope. This is a picture of the business end of it. There is a portion that is outside the picture at the top, where the electrons are accelerated and collimated and focused. The apparatus which permits you then to examine material at the level of looking at one atom at a time is in the stage at the bottom.



VIEWGRAPH 30

This is the National Synchrotron Light Source [NSLS] at Brookhaven National Laboratory. The next viewgraph then shows what is inside this building.

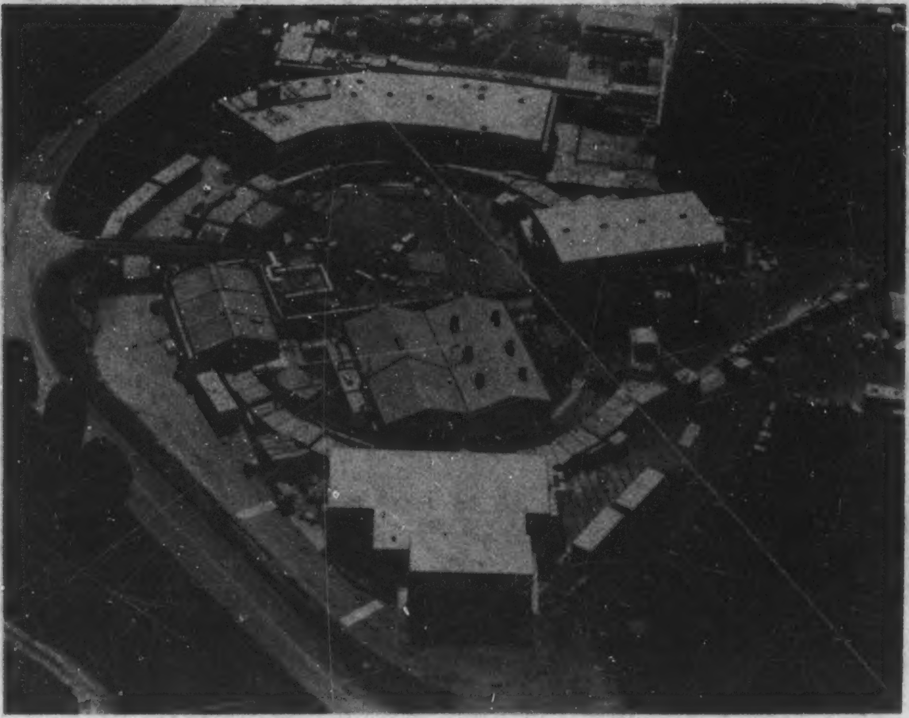


VIEWGRAPH 31

This is one of the NSLS rings. The small pipe which you see in the middle contains a beam of electrons that has been accelerated to high speed and guided around in a circular orbit.

As they go around the orbit each time, they make a bend, and they spray out some radiation. That radiation is then collimated through various pieces of apparatus that permits it to be used for experimental purposes.

It is now in operation and you will hear something more about that next week, I believe.



VIEWGRAPH 32

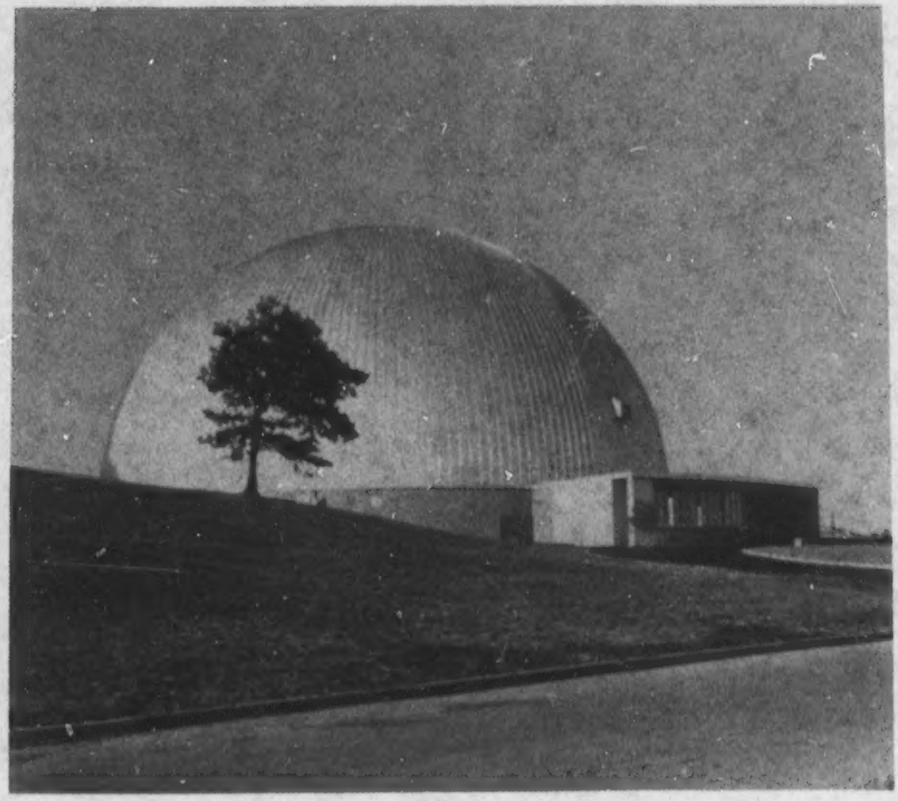
There is also a synchrotron light source that is attached to the Stanford Linear Accelerator Center. This is the so-called Stanford Synchrotron Light Sources. I want to point out these facilities do not all produce radiation in precisely the same energy range.

They are complementary facilities. They are not simply duplicates one of the other.

VIEWGRAPH 33

It is interesting that flame is one of the first things that man probably ever invented. In a flame we have a chemical reaction and yet the fundamental physics and chemistry of flames are very poorly understood. The National Combustion Research Facility at Sandia in Livermore provides a high power laser which is utilized by various users; industry, universities, and laboratories, to explore the specific chemistry of combustion in an attempt to understand better how to utilize it for various practical purposes, such boilers or internal combustion engines.

General Motors and other companies use this kind of a facility.

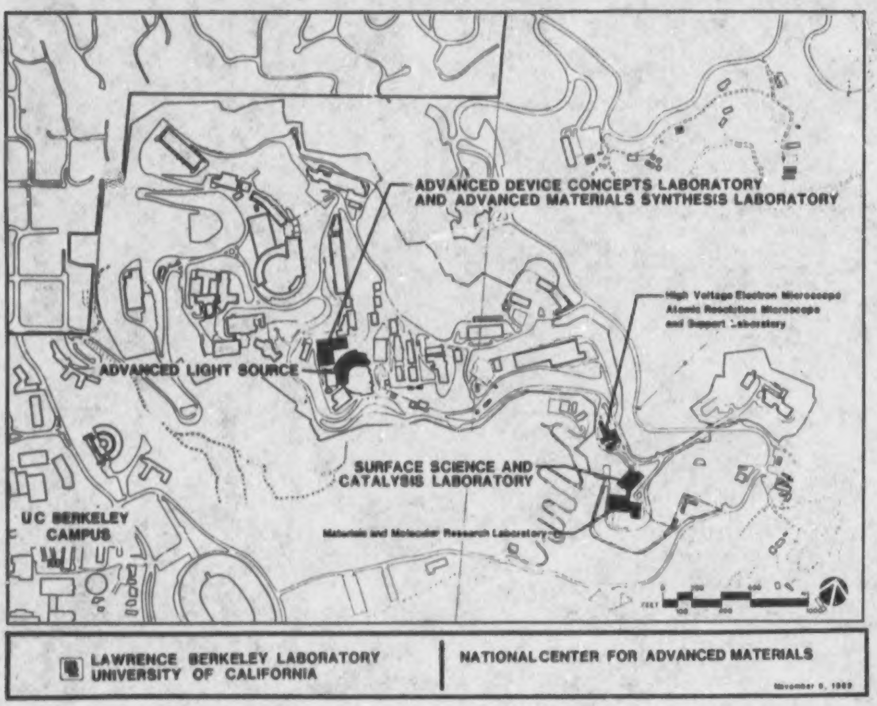


VIEWGRAPH 34

This is a picture of the outside of the containment igloo of the High Flux Beam Reactor which is located at the Brookhaven National Laboratory. It has recently been upgraded to operate at 60 megawatts. As evidence of the fact that industry does use and is interested in these kinds of things, I would like to acknowledge here that the Exxon Corp. was very generous in providing $500,000 in an unrestricted gift for utilization for this facility in order to provide better research opportunities.

VIEWGRAPH 35

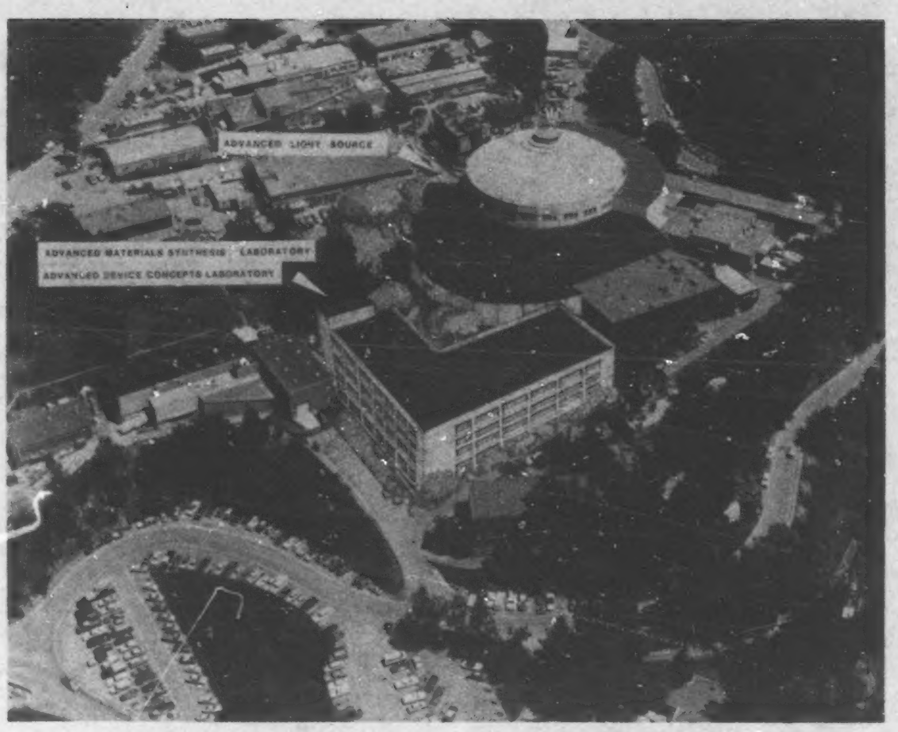
This is a picture of the High Flux Isotope Reactor, which is at the Oak Ridge National Laboratory. Its purpose is to produce isotopes that are used in various areas of research.


ADVANCED DEVICE CONCEPTS LABORATORY
AND ADVANCED MATERIALS SYNTHESIS LABORATORY
High Voltage Electron Microscope
Atomic Resolution Microscope
and Support Laboratory
ADVANCED LIGHT SOURCE
SURFACE SCIENCE AND
CATALYSIS LABORATORY
Materials and Molecular Research Laboratory
UC BERKELEY
CAMPUS
FEET
0
100
200
400
600
1000
LAWRENCE BERKELEY LABORATORY
UNIVERSITY OF CALIFORNIA
NATIONAL CENTER FOR ADVANCED MATERIALS
November 8, 1989

VIEWGRAPH 36

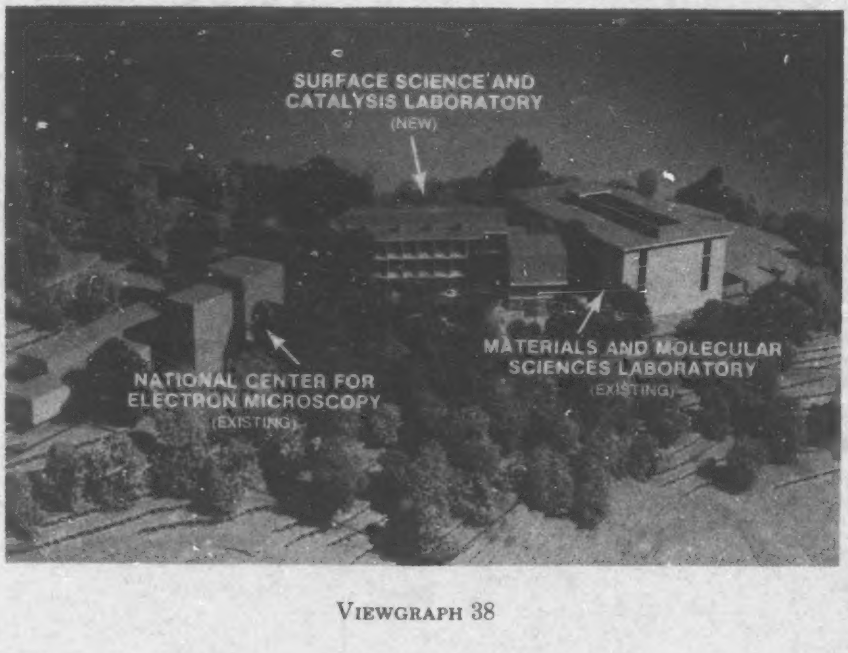
This is a map of the area around the Lawrence Berkeley Laboratory.

What is shown in yellow are the things that will be added as a result of the creation of the National Center for Advanced Materials. The materials research facilities that are there already are shown in black.


ADVANCED LIGHT SOURCE
ADVANCED MATERIALS SYNTHESIS LABORATORY
ADVANCED DEVICE CONCEPTS LABORATORY

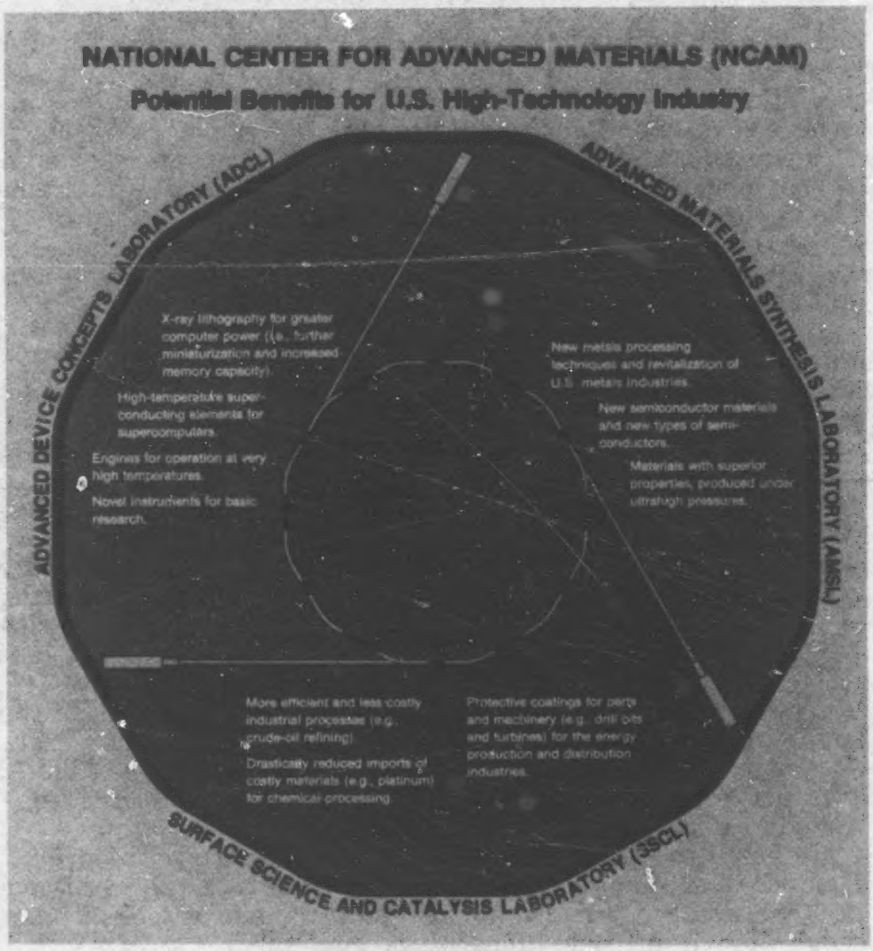
VIEWGRAPH 37

The next viewgraph shows an artist's rendition of how the old 184-inch cyclotron building will be modified in a way to allow the advanced light source ring to be located in the middle and the various laboratory facilities located around its outside.


SURFACE SCIENCE AND
CATALYSIS LABORATORY
(NEW)
NATIONAL CENTER FOR
ELECTRON MICROSCOPY
(EXISTING)
MATERIALS AND MOLECULAR
SCIENCES LABORATORY
(EXISTING)

VIEWGRAPH 38

This is an architect's version of the old buildings and the new buildings that will come into existence at Berkeley as a result of the creation of the center.



**NATIONAL CENTER FOR ADVANCED MATERIALS (NCAM)**

**Potential Benefits for U.S. High-Technology Industry**

ADVANCED DEVICE CONCEPTS LABORATORY (ADCL)

X-ray lithography for greater computer power (i.e., further miniaturization and increased memory capacity).

High-temperature superconducting elements for supercomputers.

Engines for operation at very high temperatures.

Novel instruments for basic research.

ADVANCED MATERIALS SYNTHESIS LABORATORY (AMSL)

New metals processing techniques and revitalization of U.S. metals industries.

New semiconductor materials and new types of semiconductors.

Materials with superior properties, produced under ultrahigh pressures.

SURFACE SCIENCE AND CATALYSIS LABORATORY (SSCL)

More efficient and less costly industrial processes (e.g., crude-oil refining).

Drastically reduced imports of costly materials (e.g., platinum) for chemical processing.

Protective coatings for parts and machinery (e.g., drill bits and turbines) for the energy production and distribution industries.

VIEWGRAPH 39

Finally, this is just a schematic showing the advanced light source in the center, and the fact that the various activities that will be surrounding it and utilizing the beam lines from it will be dedicated to different purposes, such as the study of catalysis and creation of new materials and new electronic devices.

Mr. Chairman, that completes this portion of my presentation.

What I wanted to try to do was to give you some idea of the size and scope of these objects that we deal with because the budget is rather large, and I think perhaps new members of the committee may not appreciate where these things are and how big they are. Certainly I would be glad to provide a longer version of this show at some time, if you would like, and certainly help make any arrangements or trips to any of these facilities that might interest you.

Thank you.

Mr. FUQUA. Thank you, Dr. Trivelpiece. We do appreciate that.

On a general subject, you mentioned in your testimony the commitment to open up the labs more for industry, graduate students, and other experimenters to do research there.

One of the things that we just finished in this committee this week was a science and math education bill. We started on it about 2 or 3 years ago, and after a longer series of hearings, we determined that not only are there problems in the graduate programs but also in elementary and secondary education.

We had Dr. Knapp, Director of the National Science Foundation, before us yesterday. I want to repeat to you what I told him yesterday, and he agreed.

We have a national problem, and on this subcommittee and this committee we were going to look more closely at the cooperation that the labs and all these programs are to help fulfill this national commitment, through outreach programs, to bring high school students into the labs. We want to let them see what is happening, to open up labs in the other agencies for people to participate in, so that we can broaden the interest of those who have a desire to pursue a career in science, math and engineering.

I hope that meets with your approval, because we are certainly going to look very closely at that possibility. I know some of the labs are already doing things, and I have been very pleased with those labs and I want to commend them. But I think all of the labs certainly need to try to make a greater effort.

It doesn't take money. It just takes a little effort on behalf of the personnel involved to participate in some of the summer math camps or summer science camps that we may have, to be able to help in an overall program of promoting science, math and engineering education.

We are going to take a hard look at that as part of the overall performance of some of these programs. I hope that the word gets out that we are concerned and are going to take a look at that when these programs come up each year for review.

Dr. TRIVELPIECE. Fantastic. How can I help?

Mr. FUQUA. Well, I think you could pass the word, too.

Dr. TRIVELPIECE. I will do that.

Mr. FUQUA. I think this certainly can go a long way in addressing the overall problem that we are faced with. I don't think I have failed to talk to a scientist yet who hasn't expressed concern to me about this. I am not being critical, but it is a problem that has evolved over the years. The government has been lax in addressing it.

I think we recognize what the problem is. One of the ways of solving it is through a kind of outreach program, whereby we make more that is going on available and not keep all this information to ourselves.

I am really speaking of high school students, high school and elementary science and math teachers, so that their interests can be rekindled, and hopefully that part of this problem that we have had over the years can be solved. I appreciate your support.

Dr. TRIVELPIECE. I think it is important. As I know Secretary Hodel pointed out to you in his testimony, that stability, continuity, well balanced programs, bipartisan support, and a certain degree of freedom from the kind of vagaries that budgetary fluctuations produce for these kinds of laboratory programs is essential. It is almost more important that they be stable than that they have sudden increases and sudden decreases.

Mr. FUQUA. Correct.

Dr. TRIVELPIECE. Because there is a tendency for the bad to drive out the good in those kinds of fluctuating circumstances.

Mr. FUQUA. Mrs. Schneider.

Mrs. SCHNEIDER. Would the chairman be so kind as to yield a moment, please?

Mr. FUQUA. Certainly.

Mrs. SCHNEIDER. As we are carrying on this discussion, there is mutual agreement that there is the need to continue the educational programs, particularly in these energy-related fields. I can't help but notice that the energy education and training program, which is part of the university research support program, is to my way of thinking going through those budget fluctuations right now that you are referring to.

It is my understanding that that has been a very successful program, whereby 75 percent of the participants have gone on to get degrees in both engineering and the sciences. It is also my understanding that this particular program was developed for the purpose of attracting women and minorities into the energy-related careers.

I look at the budget figures and I see that the funding there has been reduced from $1.7 million to $200,000 in fiscal year 1984. That really doesn't jibe with what the chairman and you are discussing now. It seems to be a move in the opposite direction.

Dr. TRIVELPIECE. The Department of Energy supports education in a variety of ways. We probably support an estimated 5,000 to 7,000 graduate students. We also have certain kinds of fellowships. We also are proposing a new initiative of $6 million for university instrumentation.

There, indeed, is some discretionary choice within that area, and we have chosen not to continue some of the fellowship areas this year. I don't think that that is producing a major difficulty, but I would certainly be glad to try to discuss it further with you, or provide some answers for the record, if you would like.

[The information follows:]

The principal activity in the Energy Education and Training program has been support for summer institutes for high school science and mathematics teachers on energy-related subjects. Fifty-five such institutes were supported in fiscal year 1982. No funds have been requested to continue this effort in the fiscal year 1983 and fiscal year 1984 budget requests. This type of precollege education program is considered to be more properly the responsibility of the National Science Foundation and the Department of Education.

Funds have been requested in the fiscal year 1984 budget ($200,000) to provide continued support for 15 projects in the Department's Precollege Engineering Freshman Program (PREP). The purpose of this program is to identify promising minority and women students in high school who are interested in pursuing engineering as an academic and professional career. Special instructional assistance in mathematics and science is provided to the selected students through a local college or university. As Mrs. Schneider indicated, over 75 percent of the students involved in the PREP program go on to college and pursue engineering or science-related degrees. This program will be continued in fiscal year 1984 at about the same level as in previous years.

Mrs. **Schneider**. I really would appreciate it if you would provide us with an explanation, because all indications seem to point out that it is a very effective program. I mean when you get 75 percent of the participants to do what the program intends the parties to do, I would say that that looks pretty successful.

You know, overall, the university and national laboratory cooperative research program, too, has been reduced from $8 million to $5 million, $8 million in 1982 to $5 million in 1984. I wonder if you could please provide this committee with the reasons as to why you are going to reduce the funding for a very effective program.

Dr. **Trivelpiece**. I don't believe the lab co-op program has been reduced.

Mrs. **Schneider**. The university national co-op program? That is the information we have.

Dr. **Trivelpiece**. The lab co-op program itself I think has maintained about the same level, but there are some other things that are carried in that line that have been modified.

Mrs. **Schneider**. Thank you very much for your indulgence, Mr. Chairman. Thank you.

Dr. **Trivelpiece**. We will provide detailed answers to that.

[The information follows:]

The program element referred to is the University/National Laboratory Cooperative Research Program with a budget request of $5 million for fiscal year 1984. This program element includes four specific program activities—the University Institutional Research Program; the University Reactor Fuel Assistance Program; Energy Graduate Research Traineeships; and the University Laboratory Cooperative Program. This latter program activity has been maintained at essentially the same budget level for the last three budget years. The program supports about 1,900 university faculty and students in summer and academic year research and training appointments at the Department's national laboratories.

The budget reductions from fiscal year 1982 to fiscal year 1984 in the overall program element reflect the following actions: (1) the completion of all projects and activities in the University Institutional Research Program ($−1.9 million); (2) one time support for engineering graduate research traineeships in fiscal year 1982 ($−1.0 million); and (3) support for a series of special minority related education and research programs funded in fiscal year 1983 with no additional funds required in fiscal year 1984 ($−.9 million).

Thus the budget reductions were not in the University Laboratory Cooperative Program.

Mrs. **Schneider**. Good.

Mr. **Fuqua**. Dr. Trivelpiece, you mentioned that for the proposed Materials Center at Berkeley that you planned to convene an ad hoc group, and that you would report to us. Do you have any timeframe when that might be? Would it be prior to the time that we would mark up the authorization bill?

Dr. **Trivelpiece**. No, I don't believe we would be able to do that. I don't have a membership in mind yet. I have some ideas along those lines and it will be as soon as possible, but I think the hearing season may dominate my life for a few weeks.

Mr. **Fuqua**. I can appreciate that.

At the Lawrence Berkeley Lab, are they proposing to do work on substitute materials?

Dr. **Trivelpiece**. Things like carbon fibers?

Mr. **Fuqua**. Materials that we can substitute for some of the metals?

Mr. **Trivelpiece**. Not precisely. They are more concerned with trying to understand, say, the subject of how—catalysts, materials used in refining process that are not used up—cause various chemicals to combine in new ways. There is a large industry that takes advantage of that, and yet the precise physics of it is not very well understood.

What we are talking about here is more toward the basic end of the spectrum of research studies as to why some of these things occur. If we understand why, perhaps we can utilize them better, but not in the sense of trying simply to create materials such as fiber-fiber type things. I think that is probably being well done in the aircraft industry, but I am not sure. Perhaps Dr. Kropschot would care to add something on the subject of substitute materials.

Dr. **Kropschot**. The Committee on Materials [COMAT] is the major coordinating group within the Federal Government. A great deal of composite and polymer work going on in NASA's program is not a major thrust of the new center. The new center will address problems in structural ceramics, new catalysts which could replace platinum, new alloys and polymers for coatings, to give you a few examples.

Mr. **Fuqua**. The committee has been very interested in materials policy. In the last Congress we passed legislation to develop a coordinated policy for materials, and particularly for materials research, and the idea of substitutes or interchangeable materials to be used rather than, say, cobalt or other strategic materials that are not only very expensive but also the supply is somewhat precarious at certain times. That was the purpose of that legislation.

Dr. Leiss, I wanted to ask you a question that you touched on. I see Dr. Panofsky in the back. I don't want him to have heart failure at this question, but it concerns the timeframe for the linear collider at Stanford. You said that it is an accelerated timeframe. Could that project be accomplished in 4 or 5 years rather than 2 or 3 years?

Dr. **Leiss**. Certainly, sir, it can be accomplished in a longer period of time. Whether it would be done for less money I seriously doubt. I think it would end up costing more. Projects of this sort do have a natural pace at which enthusiastic people who want to build them can build them.

Mr. **Fuqua**. Is there another reason besides optimum construction schedule?

Dr. **Leiss**. Clearly there is also another reason, sir. The Europeans, as we know, are building the large LEP facility, which will probably be completed in 1988 or 1989. Stanford, in addition to proving a new concept for accelerators of the future, also wants to get at the important research that can be done with that machine, if they are successful, as I think they will be, and obviously would like to make the discoveries first, if they can. We can't deny that. I think that is part of the driving motivation to the scientists there.

Mr. **Fuqua**. Another question along the same lines concerns the next generation of accelerators. The recent discovery at CERN, was by an international research team. Most of these discoveries involve international teams. Are we doing anything for a possible international program? These facilities are very big ticket items.

Dr. **Leiss**. Yes.

Mr. **Fuqua**. Are we doing anything where we can pool resources with other participating countries that are interested in this type of work, and can we participate further in any of these international teams?

Dr. **Leiss**. I would like very much to speak to that subject. As you say, the facilities are very large, and they have been growing in cost both in this country and other countries. We do pay a great deal of attention to be sure that our machines have an appropriate complementary tie to machines in different countries.

We also do a great deal of collaborative research with countries such as the Soviet Union, People's Republic of China, and Japan. We have formal agreements, because you need formal agreements to facilitate cooperation.

With Western Europe we do not have formal agreements nation to nation, but we have very extensive collaboration. For example, the team of physicists at CERN that discovered the W particle is actually led by a professor of Harvard University, who happens to also be a CERN staff member. This collaboration between scientists in different countries is very real and it is growing.

One of the major detectors that has been approved for the LEP machine, the so-called L-3 detector is an example. The leader of the scientific group involving 10 countries and maybe 100 American scientists is Dr. Sam Ting of MIT. I think this internationalism of the activities is very proper and growing. It is a step along the way to handling the problem of the increased costs of the facilities and their research.

I think we are not very far from another step that we have to consider, and a fair amount of work has been going on on the subject of the machines. People are thinking about this right now, about what is needed for advances in machines in the midnineties or the year 2000, machines that are probably going to cost appreciably in excess of $1 billion, and that may well strain the capabilities of any one country or region to build them.

So there are efforts and discussions underway, in trying to start the process of putting together an international framework to build or to support the building of such machines.

The internationalism on the utilization of facilities is an active thing at the present time.

The political hurdles that have to be solved in building them, building the machines and supporting their construction together, is a much larger political hurdle. The first steps along the way to that have been taken, in anticipation that we clearly have a future problem here that we have to contend with.

Mr. **Fuqua**. You could also use that on shared time, though, could you not, where it could be built in one country. They would pay for it, be the manager of it, but then users would have a cost arrangement or reciprocity; if you use our machine, charge time is so much.

Dr. **Leiss**. In a sense we do that. The experience that we will have on the LEP facility is in that direction. We, in our planning, are planning over a period of 4 or 5 years to expend on the order of

$25 million to help build equipment for use on detectors that American scientists would be involved in.

Europe would continue to pay the operating costs of the machine, but we would contribute a fair share of the cost of building the equipment. We have had discussions with the director of CERN to assure that we would get an adequate return on machines that are utilized. So we are moving along in those directions.

If we start talking about the cost of operating the machines, then that adds additional complications. But I think all of these things are things that we have to explore and consider, because the costs are very large.

Dr. **Trivelpiece**. Could I add something, please? I think it is a matter of tradition, in effect, that the building country operates the facility and pays for the entire operating costs. Then on a scientific merit basis, groups propose to use that facility. If one of those groups includes Europeans, then they tend to put together the entire experiment, and bring the experiment to the United States. It is then also used by U.S. citizens, but heat, light, and various other kinds of ancillary apparatus is made available for the visiting scientists.

That has been the tradition in the past. As Dr. Leiss is saying, it may well require that we modify that tradition in some way when we start talking about these next generation of experiments, so that there is a more cooperative treaty arrangement that permits sharing of costs at even the construction level.

Mr. **Fuqua**. So you are exploring those possibilities?

Dr. **Trivelpiece**. Yes.

Dr. **Leiss**. We are exploring those. I might add one point in here. We believe the systems that are working now will continue to work as long as both sides have something to offer. In other words, if we have facilities that they would like to use, and they have facilities we would like to use, we are going to have good cooperation. If we get to a position where the United States does not have facilities that are attractive to others, then we might find that we have a different situation to deal with.

Mr. **Fuqua**. Dr. Kropschot, in the basic energy science program, what are you doing that would be compatible with the fossil, solar, conservation, and geothermal programs that we have in the other parts of DOE?

Dr. **Kropschot**. Mr. Chairman, each year we pull together a crosscut of our program to see what impact basic research has on the various technologies. It is another way of looking at research. There is a long list. The way that is pulled together—and the information I can supply—you come about as follows:

We ask ourselves and our principal investigators: If you are successful in your experimental research what areas of technology will you expect to benefit the most? And in some cases like photochemistry and photoelectrochemistry the answer is fairly clear. It is solar. In certain areas of coal combustion, coal chemistry, that is also clear. We have a fairly large program in superconductivity that has multitechnology impact, and then there is a class of the longer term science that it is hard to categorize. I would be glad to provide a percentage breakdown for the record, or deal with any specifics that you have in mind here.

Mr. **Fuqua**. If you will provide that list, I think it will be very helpful.

Dr. **Kropschot**. Yes.

[The information follows:]

In our current program, the approximate technology cross-cut is solar 10 percent; fossil 10 percent; conservation (including heat engines) 10 percent; geothermal, environment and safety 5 percent; multitechnology 20 percent; fission and fusion 15 percent; and long-term advancement of science 30 percent.

Mr. **Fuqua**. Dr. Trivelpiece, recently NSF devised a different accounting procedure for some of their research programs with universities that eliminated a lot of paperwork. They have estimated that they got somewhere in excess of another $1 million worth of research from procedures relating to overhead costs and some of the other indirect costs associated with research.

Is DOE looking at that through, say, the use of grants rather than contracts?

Dr. **Trivelpiece**. We use contracts rather than grants. There is a historical reason for it, but I think it is also appropriate. DOE is a mission-oriented agency. Its origins stem from the Atomic Energy Commission.

The kind of special research contracts that are used are, for practical purposes, not significantly different from grants, except that we do try to spell out approximately what it is that is agreed upon by the faculty member and the department, in terms of what we expect them to try to accomplish. But the amount of paperwork and redtape, although they complain, I don't really think is significantly different.

I have been both an NSF grant recipient and I have been an AEC and ERDA and DOE contract holder, and I don't find any significant difference between them at the level which, as the principal investigator, I perceived them. There may be some specific advantages to one instrument over the other, but by and large how we use the contract should not cause a great deal of difference. But we are taking a look, generally speaking in the department, at the paperwork requirements as they influence the outside world. We are trying to simplify, streamline, and reduce those paperwork requirements.

Martha Hesse, the new Assistant Secretary for Management and Administration, has dedicated herself to trying to reduce the paperwork by a substantial fraction, and I hope she is successful in that. We are trying to help her identify those areas where things could be streamlined. If contracts could, we will take a look at it. I am not precisely sure what the National Science Foundation did, but it sounds good.

Mr. **Fuqua**. That is good. I am glad to hear that.

There is about $6 million in the budget request university instrumentation program for fiscal year 1984. How do you view this program, whether it is going to remain static or maybe have some growth, in the future?

Dr. **Trivelpiece**. Of course, this is an initiative at this time, and whether or not it will be an ongoing program I think remains to be seen. It will be determined in part at least by how well it succeeds in the sense of what we do with the resources that are proposed, should they be appropriated. We would try to identify items that I think are of interest to DOE.

As you understand, this is not to supply the $3,000 or $4,000 items for universities. We think that is pretty well covered in the present structure. The items that cost $5 and $6 million somehow end up being proposed in a way that those that are worthy, seem to rise to the top. There is an intermediate level for which this particular kind of funding is being requested; namely, those things that cost somewhere in excess of $100,000, and probably less than $1 million. They are used by more than one group, and they generally exceed the resources that can be made available for one program area. We would expect they would be used by several people at one university.

Obvously in our part of this, we are thinking of those kinds of instruments that would be useful and appropriate for a DOE type of research, not necessarily for research that would be, say, supported by the Department of Defense.

DOD has its own funding initiative in this area to try to improve university instrumentation for those projects that are of interest to them.

I would hope that if it is successful, given my understanding of the sort of backlog of erosion of university equipment, that this instrumentation support would grow as a function of time. But I really would like to withhold judgment on that until we have had a chance to see how well it does if it is funded this year.

Mr. FUQUA. It is a rather shocking experience sometimes to walk through one of your DOE labs and then go through a university lab and compare the equipment that they are working with. People in DOE-supported labs ought to be very proud.

Dr. TRIVELPIECE. I think this is a long overdue step, and I hope that it receives support throughout the entire budget process.

Mr. FUQUA. Thank you. Mr. Bateman.

Mr. BATEMAN. Thank you very much, Mr. Chairman.

If I might, at the outset I would like to compliment Dr. Trivelpiece and his colleagues. Your testimony has certainly exuded a manifest confidence and enthusiasm for what you are doing that I am sure all of us who heard you appreciate. It is very comforting to get that sense of not only dedication but effectiveness in what you are doing.

If I may, I would like on behalf of Mr. Carney to relay a request, in view of his inability to be here by virtue of being called to the Pentagon this morning. His request is that he be permitted to submit a statement and questions for the record concerning the colliding beam accelerator at the Brookhaven National Laboratory, and I would like that request noted for the record.

Mr. MINETA [presiding]. Without objection, it is so ordered.

Mr. BATEMAN. If I may, I wonder if you could help me understand somewhat better some technical dimensions of high energy physics activities.

I have read in testimony and in other materials that I have seen references to TeVs, GeVs and MeVs in the context of electron accelerator laboratory facilities, and I am having difficulty quantifying which is bigger, better, faster, a TeV, a GeV, or an MeV. Could anyone shed any light on that for me?

Dr. **Trivelpiece**. I will give that a try. An electron volt is simply a unit of energy. You can think of it as the unit of energy like a calorie or anything else. These can be converted from one to another. One electron volt is the unit of energy that most physicists use.

In view of the problem with writing lots of zeroes, there is a scientific notation that allows uses. Ten raised to some power to express big numbers. We use prefixes to describe them. Ten raised to the sixth power is one million, for which the prefix is MEGA, abbreviated to M. Ten raised to the ninth power is one billion, for which we use the prefix GIGA, or G. Ten raised to the twelfth power is a million million, which has the prefix TERA, or T. So a MeV is a million volts, a GeV is a billion volts, and a TeV is a million million volts.

Mr. **Bateman**. So it goes M, G, T: Tinkers to Evers to Chance, or something.

Dr. **Trivelpiece**. Essentially each one is a thousand more than the previous one.

Mr. **Bateman**. I believe you have essentially covered this question that came to my mind, and that is the funding sources of the CERN Laboratory. I take it this is a multinational Western European facility?

Dr. **Leiss**. Yes, sir.

Mr. **Bateman**. Available to or with some participation from the American scientific community?

Dr. **Trivelpiece**. Yes.

Mr. **Bateman**. Their accomplishment in the identification of the W particle—what was critical to that other than the brilliance of the scientists in terms of the instrumentation?

Dr. **Trivelpiece**. As I pointed out when I started you have a proton and an antiproton which have to collide. That is the first event. Contained within those two objects are quarks and antiquarks, and they have to collide in such a way that they annihilate. As they annihilate, they then have to create new matter from energy.

Since energy and matter have to be conserved, you have to have enough energy available to create this particular object, the W particle. The W particle has a mass that is about 80 or 90 times that of a proton, so if you cause two protons to come together, you have to cause them to come together with enough energy so that there is energy left over when these quark and antiquark annihilate to cause this new particle to appear literally out of thin air. So it is the energy requirement that is crucial in this.

You have to have enough excess energy left over for this particular object to come into existence, and their accelerator is the first accelerator that has in the so-called center of mass, namely, the framework in which this particle is going at the same speed as this other particle as they collide. That center of mass energy has to be above the threshold, and they have achieved that threshold and have carried out these experiments.

Mr. **Bateman**. If one were to grade or quantify their accelerator at CERN, would you do it in terms of GeV's or TeV's?

Dr. **Trivelpiece**. 540 GeV is the collision or center of mass energy, 270 for the particles going in one direction, the protons, and 270 for the antiprotons going in the other direction.

Mr. **Bateman**. Mr. Chairman, if my time permits, I would like you to try and put that into the context of Dr. Peter D. Barnes' Electromagnetic Interaction Subcommittee on the Nuclear Science Advisory Committee's recommendation for a 4 GeV CW electron accelerator to be constructed.

Is that a facility that you gentlemen regard as being needed and desirable in order for the American scientific community to be able to work its wonders?

Dr. **Trivelpiece**. It is a slightly different kind of facility than those we have been previously talking about. Having a proton collide with an antiproton requires one kind of facility to explore matter. Another way by which you can explore matter is to have electrons impact with protons, and to discover what happens then.

This particular device is one which would cause a steady current of energetic electrons to collide with target protons in order to explore their internal features, or the internal features of nuclei, of atoms.

The desirability of that facility, and the advisability of it is being explored, as you say, by the Nuclear Science Advisory Committee. They, I think, are enthusiastically for it. It is clearly recognized in the nuclear physics community as probably the next most important facility that should be built.

There will certainly be a lively debate on which proposal might be the most worthy among those currently being considered, but I think from my point of view it is certainly the next most important facility for the nuclear physics community to give consideration to building.

Perhaps Dr. Leiss would like to add something to that.

Dr. **Leiss**. I would like very much to support that position. I should say, as I commented in my testimony, that by putting the relatively inexpensive injector most of the way down the SLAC facility, we will be able to get a first glimpse at the research that people believe is in that region, and guide the research program with that. But we do have several proposals from institutions proposing to build the facility which you described.

We have for several years had various studies looking at what was the right machine in this region, what was the physics potential or the important questions that need to be answered. We have been supporting R&D, so we were sure we had the technology to build that machine. So we believe that facility is a very appropriate thing that we would hope to bring in front of you.

Mr. **Bateman**. That evaluation process, among those who would like to be selected to implement their design, and for the construction and operation of the facility, is ongoing?

Dr. **Leiss**. Yes, sir. We have a Nuclear Science Advisory Committee, and a subcommittee is active right now reviewing the five proposals for machines that we have received on this.

They met Thursday, Friday, and Saturday of this last week, and heard presentations on the proposals. They will have a recommendation to the Nuclear Science Advisory Committee probably April or May of this year, who will then recommend to us what they believe is the sensible facility plan for this area of physics.

Mr. **Bateman**. And these various alternative designs are of a new facility or a type of facility that the physics community of the United States looks upon as being critical to their short and longer term research needs, and not duplicative or overlapping of other existing facilities?

Dr. **Leiss**. Yes, sir. Our problem is that we don't have a machine in the world of the energy and continuous stream of electrons that we need for the research.

Mr. **Bateman**. Is the key to this the distinction that it is a continuous flow of energy as opposed to energy that is souped up and used to zap something and then it is over?

Dr. **Leiss**. Precisely. The research you want to do requires you to find out not just how the electrons scatter, but what else came out at the same time. If it comes in two intense bursts, you just have so many things happening at one time that you can't sort out the results. So the continuous nature is the thing that we don't have at the present time.

Mr. **Bateman**. Thank you, Mr. Chairman. I will yield back any remaining time to my colleagues, and perhaps will return for a later round.

Mr. **Mineta**. Fine. Thank you very much, Mr. Bateman. Mrs. Schneider.

Mrs. **Schneider**. Thank you. I very much appreciate your testimony and your responses to some of the questions my colleague Mr. Bateman asked, which deal with the scientific angle of things, but our responsibilities here are budgetary. I have a number of questions that relate primarily to the budget.

I am curious to know, insofar as the ERAB report is concerned, if there has been any following of their recommendation about designating specific laboratories as lead laboratories for specific DOE programs.

Dr. **Trivelpiece**. Lead laboratories are already a part of the DOE management structure. They are utilized for certain appropriate programs. They are not a universal panacea for managing the entire Department of Energy, but I did talk about this a little bit in the testimony that I gave earlier in December before this committee on the department's process of reviewing the ERAB Lab report and what steps we are taking to review those recommendations, and how we will let the Secretary get involved in certain of the controversial issues.

But with regard to the lead lab concept, it has been used for some time. It is not something that is new. I think ERAB was suggesting that there may be places that we are not now using it for which it would be appropriate, but that needs to be explored on a program-by-program basis.

Mrs. **Schneider**. And you are looking into that now?

Dr. **Trivelpiece**. Yes.

Mrs. **Schneider**. On a program-by-program basis, fine.

Another one of the recommendations from that report was that there needed to be more emphasis on public health concerns, and also environmental concerns, such as acid rain. I was wondering if you had picked up on that recommendation.

Dr. **Trivelpiece.** Acid rain in the ERAB Lab report? I don't recall that.

Mrs. **Schneider.** You can skip the acid rain, but what about just, in general, environmental health concerns? You don't have to specifically respond to the acid rain idea.

Dr. **Trivelpiece.** The health and environmental research program will be the subject of another hearing on March 24, 1983. We can certainly talk about it at that time. Yes, I believe we should have a sound health and environmental research program. The Department of Energy has some rather unique skills and facilities and, in a sense a mission and charter with respect to this area, in an attempt to try to understand the health impact of various energy technologies in such a way that you can, for instance, try to put in place some kind of mitigating strategy, or modify an energy process before it gets utilized in the consumer field in a manner which could endanger public health on the environment.

For example, I think one of the things that has happened as a result of inhalation toxicology studies is the understanding of diesel particulates and their mutagenic characteristics. If you were to take diesel particulates and grind them up and look at the individual chemical components, you would find that they appear to be rather virulent from a mutagenic point of view.

However, for whatever reasons in the less than one micron-sized particles the diesel particulate comes out in, it turns out not to be very harmful from a health point of view, because macrophages which are a form of white blood cells, in your lungs, engulf the particle and benignly remove them from your system. So exposure to diesel particulates is a much less serious problem then previously thought. So if we didn't have that kind of data from these kinds of studies, one could conclude that diesels were much more risky than they clearly are.

Mrs. **Schneider.** One of the things that I think a number of us will be doing will be to review the ERAB report to do a scorecard to see which of those recommendations you have picked up. I, for one, put a great deal of value in that report. I think that all the way through it reflects a great deal of common sense and sound judgment, and it does use, I believe, an approach toward cost-effective expenditure of our hard-earned taxpayers' dollars. So I will be reviewing that.

On the same score, I will also be reviewing to what degree you may support, for example, an investment or putting greater emphasis on health research, based on your budgetary priorities, and watching to see if those dollars are going where the recommendations are being made. I can't help but do this, due to the fact that it has been brought to our attention before this committee that there seems to be a great deal of contrast in the expenditure of dollars.

For example, a coal sample bank at your Argonne Laboratory contains, from what I understand, about five different samples of coal, whereas Penn State University, at the same annual cost, has 1,000 coal samples.

I am just wondering, how is it that the Argonne Laboratory can spend the same amount of money as Penn State University, and Argonne can have five samples and Penn State 1,000?

Dr. **Trivelpiece**. You have the advantage. Maybe Dr. Kropschot would like to comment.

Dr. **Kropschot**. I should start by saying the purpose of the coal sample banks between the two organizations, Penn State and Argonne, are completely different.

Mrs. **Schneider**. How so?

Dr. **Kropschot**. The Penn State sample is concerned with routine coal samples and the other with specifically collected, prepared and stored reference grade coal. The former differs from sample to sample and degrades in time; the latter do not. The reason that the new program at Argonne was started was in response to a workshop which identified the need for such a collection of samples for the basic research community which is looking at fundamental coal chemistry and catalysis.

Mrs. **Schneider**. Excuse me. And what?

Dr. **Kropschot**. And catalysis. And what we need for these programs are well-characterized material that will feed a characterized material into the experiment. If you have a sample of copper or gold, you know what it is. If you have a sample of coal, you are not sure what it is. It can be lots of things. What we need for the basic research community are characterized samples that are dependable, so that when the scientist makes his measurements, he knows that he is making it on repeatable and dependable characterized materials, and can go back to that same kind of material in the future to see whether it is the experiment that is changing or whether it is dependent on the feedstock, if you will, the coal that goes in.

The sample bank at Penn State is a large-volume sample bank that has been in existence for many years. It has served the community well by providing quantities of nominally the same material. Such samples are inexpensive and are very suitable for the more applied research community. However, for the more fundemental reseach, the samples lack reproducibility and chemical stability. So there are different objectives for the two programs. The premium coal sample bank was a specific recommendation of a March 1981 Coal Sample Bank Workshop which was co-sponsored by DOE and the Gas Research Institute in which the need for this unique collection of coals was documented in the context of all the other existing coal banks in the United States.

Mrs. **Schneider**. We will be pursuing that, I believe, in hearings at some point later on, perhaps with a Penn State University explanation side by side. But it is imperative for this committee to have that kind of information, because this is an example where on the surface, we would appear to be funneling large amounts of money into a national laboratory, whereas it appears, as I say, at first glimpse that one can attain this same goal with the expenditure of fewer dollars.

We are concerned before this committee that, particularly during this high-tech era, there seems to be the adoration of science and all things that require a Ph.D. and then some, and it is a difficult responsibility that we have to make those determinations, not just to stand in awe of the science that you are doing, but with as much capability as we have individually to come up with the appropriation decisions, and there is great concern now that there is a tendency to throw money at these kinds of investments. What we want to do is to look at cost-effective studies like the ERAB report and other recommendations and examples, and try to make the best possible decisions for the budget, without jeopardizing our future lead in science, discovery and development.

So we would be most appreciative of any additional material that you can supply us with, because we are going to be most critical, seeing as how we are looking at an enormous deficit, and we are also looking at a tendency of many members to just throw money at programs such as this.

Dr. **Kropschot**. If that is your question, I missed one key point.

Mrs. **Schneider**. It was more of a statement, excuse me.

Dr. **Kropschot**. Let me make one last point. When the Argonne program originated, it was a collaborative effort of our program in basic energy sciences, the fossil energy program and the Gas Research Institute in Chicago to develop this new coal sample bank, I believe I am correct in saying that Penn State University was asked first—I know they were asked—to see whether their coal sample bank would do the job that we were trying to accomplish. The answer was no.

Penn State was considered as part of a joint program with Southern Illinois University. The Pittsburgh Energy Technology Center ORNL, and Argonne were also considered before a decision was made to select the Argonne proposal. The National Bureau of Standards was also considered but they were not interested. The Argonne proposal was peer reviewed by a panel of experts from the coal research community and accepted by them before the Argonne project was undertaken late in fiscal year 1982.

Mrs. **Schneider**. You went through that process?

Dr. **Kropschot**. We did go through that process.

Mrs. **Schneider**. Thank you very much. I really do hate to ask questions and then run, but I have another hearing that has been going on for 1 hour and 15 minutes now, so I thank you for your very enlightening testimony. Thank you, Mr. Chairman.

Mr. **Mineta**. Thank you very much, Mrs. Schneider, for your contribution.

Dr. Trivelpiece, I apologize for not having been here earlier. As Mrs. Schneider is running off to a hearing, I had been at another one myself.

You are familiar with the letter, I believe, that has come from Bell Labs relative to NCAM and there is another letter that came in from U.C. Santa Barbara to the full committee here. As I understand it, you are going to set up some kind of an ad hoc scientific review committee, and I understand that your response this morning to the chairman was that the work of that committee would not be completed prior to the markup of any authorization language or legislation before this committee.

I don't know if you have seen those letters, but I am wondering if the support of the NCAM facility is lessened by what you know of that correspondence.

Dr. **Trivelpiece**. Not in my view. In fact, we are not simply trying to identify a single scientific purpose and then create a facility to meet that particular purpose. This is a result of a very careful and deliberate plan to try to take advantage of the setting in which the Lawrence Berkeley Laboratory is, namely, the University of California at Berkeley, where there is a close collaboration with graduate students and faculty, and almost in some cases an interchangeability between those two institutions in the way that research can be done and students can be trained. The educational element of this is a very important one.

The other element was that since this facility was first called to our attention as a possibility in some of the institutional planning processes we asked the Lawrence Berkeley Laboratory to go through last year, we have asked them, as I have also done, to contact various industrial organizations, to try to determine what it was that they thought would be a useful ingredient or characteristic for this type of facility.

We have received a number of very enthusiastic letters from most of the major corporations of the United States, commending us to do this, recommending it, and expressing a strong desire to take advantage of it, once it has come into existence. So the industrial community, at least those other than the one scientist from Bell Telephone Laboratories, seem to be rather enthusiastically in favor of this.

I don't regard the letters as in a sense calling attention to any essential defect in the plan. In fact, the letter from Dr. Phillip Anderson says he thinks it is a good idea to redirect the Berkeley Laboratory. He thinks the general ideas are good. He is concerned about some technical details as to whether the advanced light source is precisely the right energy range, and things like that.

As I said earlier, we perhaps don't have the final word on wisdom in this thing, and certainly if there are constructive ideas to be gleaned from the scientific community that has an interest in it, we would be delighted to hear them and, if appropriate, to take them into account. But I was trying to and would like to reemphasize again, I would hope that this kind of a review would not delay the beginning phases of getting this project started. I believe that there is adequate time to take these kinds of concerns into account and either answer the questions in a satisfactory manner or make some modifications in the plan in a way that takes advantage of good ideas.

Mr. MINETA. As far as breaking it up into component parts and having this piece go to that national lab facility and another piece somewhere else, you would not see that as a way of dealing with this issue?

Dr. TRIVELPIECE. I sure don't think that is a very good idea. I think this is a circumstance where it truly can be that the whole is greater than the sum of the parts. This proposal constitutes an integrated facility and the interaction between the individuals involved in each of these facilities and activities is an important ingredient. Scattering them to the winds would not be particularly useful.

Mr. MINETA. There are three DOE accelerator centers, and I am wondering, do you expect the electric power costs at those accelerator centers to continue to escalate?

Dr. TRIVELPIECE. Yes.

Mr. MINETA. And given that, what are we going to do about trying to moderate or hold down those increased power costs?

Dr. **Trivelpiece**. I certainly hope that our economy changes in such a way that the inflation rate goes down. Should the inflation rate go down, then the cost of electricity might not escalate at the rate that it has in the past few years.

It is very clear that the escalation rates we have experienced have produced a serious problem. These facilities each use—and I forget the exact amount, but it is somewhere in the neighborhood of 40 or 50 megawatts of electricity.

Part of what we have done is to try to do things like the energy saver at the Fermi Lab, which is to use superconducting magnets in a way that, with essentially the same electricity cost, you can get higher particle energies and more efficient utilization of the electricity that is there.

One of our laboratories benefits from the electric power rates of the so-called Western Area Power Administration (WAPA). We would like, obviously, for several of our labs, to have more of that WAPA power, but it is competed for rather intensively by a large number of forceful political constituencies. It is of great benefit to us.

Cost of electricity is a significant element in our programs. I don't have any magic plan, and I don't know that anybody else does, for solving this. It is one of the costs of doing business. We do such things as develop more efficient accelerators, trying to operate during periods of off-peak loads most of the laboratory directors do rather creative things. Dr. Panofsky is here from Stanford today. He is probably one of the more creative in this respect. I am sure he would be delighted to answer.

Dr. **Leiss**. I would like to comment. Clearly the escalating power bills have been a major problem and as we go to larger machines and these machines we dream of, power bills become an even greater concern.

This is one of the driving forces that led us several years ago to decide that no matter how painful, our future depended upon our learning how to use superconductivity in the building of our machines, because without learning how, we simply did not have a future.

Now, we have gone through a long exercise on that. It has been somewhat more painful than we may even have anticipated, but as the energy saver comes on the air, I think we will demonstrate that we have invented, a whole new technology on a large scale. I think that is going to pay off in other ways, but it also pays off in being able to build and operate our machines, in an affordable manner.

To the extent we can, by scientific ingenuity, we have tried to contend with this problem in the energy saver and the colliding beam accelerator research at Brookhaven, the Atlas facility at Argonne using superconducting cavities, and the University of Washington upgrade that we are proposing, using superconducting cavities.

We are trying to solve this power bill problem as best we can.

Mr. **Mineta**. When you go through your life-cycle costing in terms of superconductivity, you find that there are long-term savings even though there may be increased power rates. Weighing the costs of the superconductor versus the increased power costs, you find in the life-cycle costing that you are ahead of the game?

Dr. LEISS. Very much so, although let me say, sir, that life-cycle costing in research facilities is a very tricky business. What we really are going to be able to achieve, for example, with the energy saver is for the same sum of money to be able to operate at approximately twice the energy and 50 percent more time. It is a productivity improvement, that is a better way to say it perhaps, rather than life-cycle costing.

Mr. MINETA. Does the DOE nuclear physics program have a clear set of goals for the next 10 years, Dr. Leiss?

Dr. LEISS. That is a tough one to answer. Certainly we have important research, important research opportunities that present themselves to the nuclear research activity. Let me give an example of the electron machine that we discussed earlier.

There has been research done at very high energies that seems to indicate that the size of the protons in the ion nucleus are a different size than those in deuterium. This is very exciting. If it really would be confirmed, this is a very exciting and important scientific realization.

It makes the desire to do these experiments with the four GeV CW accelerator enormously intense. There is a whole area of physics looking at the quark presence in nuclei and how it manifests itself and how the fact that the quarks and the nucleons are in a nucleus rather than by themselves, how that influences the properties.

There is very great interest in trying to collide very heavy ions, uranium atoms on uranium atoms, at very high energies. There is a substantial theoretical prediction that you wil produce a plasma of quark matter in the nucleus, and you don't have any other way to achieve that.

Mr. MINETA. Given the difficulty of establishing some specific goals within a given period of time within the DOE nuclear physics program, what about in the nuclear physics community; is there a general consensus as to what those goals and areas ought to be, even though it may be difficult to specifically say, yes, these are the seven things we ought to be doing in the next 10 years?

Dr. LEISS. Sir, in 1979 the Nuclear Science Advisory Committee (NUSAC) developed a long-range plan which was widely endorsed. NUSAC is in the process of developing an updated plan which I would hope they would complete during the next year.

I think we will have strong support for the consensus plan that will be developed by the community. Obviously some scientists will be more interested in one area or sub-area of nuclear physics than others, but I think that a plan requires a balance of all the various interests of the community. I think we do have a consensus of the community on what the interesting areas are.

Perhaps the more difficult problem is trying to decide where are the boundaries of nuclear physics as compared to, for example, when does nuclear physics become high-energy physics. Where are the boundaries of what should be in each program is a more difficult problem for us to wrestle with. It is an administrative matter primarily, but it still is tricky.

Mr. MINETA. Dr. Kropschot, in fiscal year 1984 the basic energy sciences program will continue modest funding for heavy ion fusion research, and I understand that this is an activity that has been transferred from DOE's defense program. If heavy ion fusion research should yield favorable results, how far would your program be willing to fund this activity?

Dr. KROPSCHOT. At the present time it is in the research stage, and it is in the basic energy sciences program. I would like to defer to my colleagues, Dr. Leiss or Dr. Trivelpiece.

Dr. TRIVELPIECE. I think there are two issues that are involved here. One is the issue of if you have a particular type of target pellet, how it behaves and compresses and produces fusion. The other is how do you supply the energy to that particular pellet. There are a variety of ways of supplying energy. One is through the use of lasers. One is through the use of light ions. Another is through the use of heavy ions.

What we are doing here is the research to try to demonstrate whether or not a beam of heavy ions can be produced and accelerated to a high enough energy to supply energy to a target that would then be capable of being compressed and undergo nuclear fusion. But what we are not doing is the second part of that, which is to have that beam applied to the target.

We are essentially going through the feasibility phase of this by trying to understand from the research point of view whether or not such beams can be created, brought to the right energy, and so on.

I think the program would have to be reviewed very carefully at that point to decide whether or not to proceed, and if it were to proceed, whether or not we would be the appropriate group within the Department to carry out that research.

Mr. MINETA. That would be with what kind of a timeframe?

Dr. TRIVELPIECE. Over the period of the next 3 or 4 years I would think would be an appropriate time scale.

Mr. MINETA. In your testimony you mentioned the goal of increasing the number of students pursuing professional-level degrees in energy-related science or engineering, and given that interest and goal that you mentioned, why has DOE not continued the energy graduate traineeship program, which was begun in fiscal year 1982?

Dr. TRIVELPIECE. This is a matter of priorities, and in that program area the guidance and interaction we have had has been such that that area has not been one that we have emphasized. I don't think that that should be taken as a lack of interest on our part to support graduate training. We do do that a great deal.

As I said earlier, we support 5,000 to some 67,000 graduate students. We don't happen to support in that particular category, although a couple of the programs do have specifically targeted fellowships for fusion and for nuclear engineering.

I think what the Department has gotten away from is what I would refer to as more broadly based type of fellowship support. Whether or not we should get back into that is a rather interesting issue, but at the moment we are not asking for funds in that particular category. That does not reflect any lack of interest in wanting to support graduate research. It is just a question whether or not that is the most appropriate method.

Mr. **Mineta**. Dr. Trivelpiece, thank you very, very much for your appearance here today. Thank you, Mr. Chairman.

Mr. **Fuqua**. I just wanted to add my thanks to you and your colleagues for being here today. We look forward to working with you throughout the year.

Dr. **Trivelpiece**. Thank you, Mr. Chairman.

Mr. **Fuqua**. Thank you very much.

The subcommittee is adjourned until Wednesday, March 2.

[Whereupon, at 11:30 a.m., the subcommittee adjourned, to reconvene Wednesday, March 2, 1983.]

# 1984 DEPARTMENT OF ENERGY AUTHORIZATION

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WEDNESDAY, MARCH 2, 1983

HOUSE OF REPRESENTATIVES,
COMMITTEE ON SCIENCE AND TECHNOLOGY,
SUBCOMMITTEE ON ENERGY DEVELOPMENT AND APPLICATIONS,
*Washington, D.C.*

The subcommittee met, pursuant to recess, at 9:41 a.m., in room 2318, Rayburn House Office Building, Hon. Don Fuqua (chairman of the subcommittee) presiding.

Mr. FUQUA. The subcommittee will be in order.

Today the subcommittee continues its review of the fiscal year 1984 budget request for the Department of Energy's basic research programs. The administration's request for these programs—basic energy sciences, high energy and nuclear physics, university research support, and instrumentation—exceeds $1 billion.

The request provides real growth for scientific research and also includes initiatives for new research facilities at the national laboratories and for new instrumentation at our universities. The proposed budget for these programs is part of the administration's policy to strengthen U.S. science in selected fields.

The committee shares the administration's view that advances in science hold the key to developing tomorrow's high technologies. We believe that the Federal Government will continue to play a major role in the support of basic research, especially at our universities.

As the DOE research budget grows, we must be certain that it is being allocated productively and fairly. In particular, we want to learn about the interactions between the national laboratories, industry, and the universities. Recent reports indicate that improvements are needed in the relationships between these three sectors in the scientific research community.

We also want to make sure that DOE is setting correct research priorities and that it is properly balancing the support for science at large, central facilities—so-called big science—with support for small science, carried out, typically, at universities.

Today's witnesses will provide us their views on the role of DOE in meeting the needs of the materials science, chemistry, and university research communities.

We are very pleased to have Dr. William Brinkman, with the Bell Laboratories; Dr. Allan Schriesheim of the Exxon Research & Engineering Co.; and Dr. Robert Rosenzweig of the Association of American Universities.

We are pleased to have you.

At this point in the record, without objection, we will insert an opening statement from the Honorable F. James Sensenbrenner, Jr., of Wisconsin.

[The opening statement of Mr. Sensenbrenner follows:]

### OPENING STATEMENT OF HON. F. JAMES SENSENBRENNER, JR.

Thank you, Mr. Chairman, for this opportunity to extend a welcome to our witness.

We look forward to their testimony today on the research programs conducted by the Office of Energy Research in the U.S. Department of Energy. These programs include High Energy Physics and Nuclear Physics, Basic Energy Sciences, University Research Support and related activities. They support long range, basic research upon which this country's future energy options will depend.

The Administration has proposed significant increases in these programs for fiscal year 1984. The Basic Energy Sciences budget is increased by approximately 14 percent above the amount appropriated in fiscal year 1983, the Nuclear Physics budget is increased by 16 percent, and the High Energy Physics budget is increased by 17 percent.

While I support the strengthening of these programs, we must be assured that these increases are warranted and represent a wise investment of our tax dollars. Your input, as representatives of the private sector and the university community, is appreciated and welcome.

Mr. FUQUA. Dr. Brinkman, we will begin with you. If you have a prepared statement we will make it part of the record in its entirety. You can summarize if you like. We will hear from the three witnesses and then we will have questions among the three of you.

## STATEMENT OF DR. WILLIAM F. BRINKMAN, DIRECTOR, THE PHYSICAL RESEARCH LABORATORY, BELL LABORATORIES

Dr. BRINKMAN. I will just summarize my written statement, Mr. Chairman.

I am Bill Brinkman, and I am currently the director of physical research at Bell Laboratories. I am here today to talk about the basic energy sciences budget for energy supply, research and development.

I come here not only because of my experience as a director at the Labs, but more because of the fact that I have served on various review committees for the Department of Energy, on review committees of the national laboratories' committees and various committees of the National Academy of Sciences.

First I would like to summarize what materials research or materials science is. It is currently a very diverse field which starts out with condensed matter physicists who work on fundamental physics questions, questions of cooperative physics. These kinds of studies are of the type that have been recognized by Nobel Prizes in the last few years.

It goes from there all the way over to people trying to grow new types of materials, materials that incorporate radioactive wastes, ceramic materials, and polymers.

Materials science in that sense underlies much of American industry and it is very, very important for long-term growth. It underlies the electronics industry; it underlies the polymer industry, ceramics, the automobile industry, and the airplane industry.

Because of that very diversity, I think it is important to recognize that it is generally done by relatively small groups of people, and thus, it is only natural that it is not synonymous with large coherent groups. Because of this priority setting is a very diffuse process. It is not a process in which you can expect a coherent group of people to come together and give you an answer.

That is different from many other fields, such as elementary particle physics, where the goals are very narrow and very well defined.

There is one area, however, where the condensed matter physics community and the materials science community has gotten together and tried to deal with setting priorities. This has to do directly with the Department of Energy's budget, and it basically involves the use of neutrons, ultraviolet radiation, and X-rays.

In my opinion, neutron scattering is an extremely important part of materials science. It offers us a type of probe of materials that is unique and cannot be found elsewhere. It is very important from this point of view. Neutron scattering is basically concentrated at four national laboratories: Argonne, Brookhaven, Oak Ridge, and Los Alamos. In recent years, some of the things you might have heard about is that neutron scattering has been used to study systems which exhibit simultaneously superconductivity and magnetism. Elsewhere, organic polymers are being studied with renewed interest using neutrons.

Another area that is important to us from the national facilities point of view is use of synchrotrons. Synchrotrons give us ultraviolet radiation and X-ray radiation at intensities that are two or three orders of magnitude greater than we ever had before. That kind of change in what we can do has had a major impact on the class of experiments we attempt with these kinds of probes.

This has turned out to be very important for us, particularly in the study of surfaces, and surface reactions, but it also has turned out to be important in molecular biology and physiology.

It seems to me that in these national facilities, the Department of Energy has played a very important role in supporting American industry. Any one American industry does not have the wherewithal or finances to fund a facility of this type. The Department of Energy plays the role in supplying us with the opportunity to use these types of probes.

The important thing that we have learned in the last 10 years in running facilities of this type is how to balance the role of the various participants, the people inside the laboratories, at the universities, and at industrial laboratories. We have had a lot of growing pains in our community as we tried to figure out how to do that. We have learned to open these facilities up so that they are open to individuals from all the different industries and universities.

In this process of learning how to run national facilities, it seems to me that the important thing has been the continued feedback of the community and interaction with the community in establishing such facilities.

This feedback and interaction has to occur in the planning stages, during construction, and after the facility is running. I believe that the Department of Energy as a whole has been very good about consulting with the community about constructing the facilities they propose. They have established committees on their own to look at these things. They have gone to the National Academy and asked for information.

Now, besides these facilities that the Department of Energy runs in materials science, it supports many the university research programs. It seems to me that that is very important in this area. Scientists are being trained directly in the field. Since U.S. industry needs people, this is a good thing to be doing.

Finally, I would like to say something about the other roles of the national laboratories that are doing materials research. When you try to think about materials research, the national laboratories come up with two distinct roles that you can identify. One of them is running of facilities. The second one is to look at what I call the medium-size science. Very often, there are techniques and scientific programs that require some intermediate-level funding. By intermediate-level funding, I mean in the million-dollar range or half-million-dollar range. Not only do they require that kind of funding, but these programs require a technical and engineering know-how that is not available at universities. You have to understand that problem.

The national laboratories offer us that kind of expertise. They have a staff which has engineering and technical know-how that can do things that could not be done elsewhere.

An excellent example of this medium-level science is what has been done in West Germany in various laboratories there, where they in fact have taken certain techniques that needed evolutionary development of equipment and managed to develop them and to make them into a really useful experimental technique.

In this country, I think you might cite the ion-solid interactive and laser annealing program that the Oak Ridge Laboratory has had as one of these intermediate-level scientific programs which I think has been quite outstanding.

Finally, I think it is important to recognize that there are things that the national laboratories should not be involved in, and that is what I call real development work. When an industry needs to make a product, behind that product, generally, lies the development of a variety of new materials. Particularly in the electronics industry, which we are familiar with and it takes an enormous effort. Many people have to be involved and the development has to be done very carefully and must be closely coupled to the commercial needs of the company.

In order to have that happen, we find even in industry that it is a very difficult process, and to have that going on at a national laboratory, where the connection is even more remote, seems to me not a very profitable thing to be doing and is not the right thing for national laboratories to do.

What they can do is basic research that feeds into the development process whereby an industry can look at these new ideas and make development decisions.

Finally, I will just close by saying that in order to insure that programs such as those just described that involve relatively small amounts of money from a national point of view are initiated, it is essential that the climate in Washington should not be one where the only new initiatives that can be supported are those that cost hundreds of millions of dollars. One sometimes finds that it is easier to fund a large program, often with the simplest of goals, than to sell the continuous flexible funding of science where priority setting can be made at a level at which knowledgeable decisions are possible.

In conclusion, I believe the materials science program at the Department of Energy is an important part of our national scientific effort in an area that is crucial to the long-term health of U.S. industry. The national laboratories have a crucial role to play in this effort, and it is encouraging to see the recognition given to this fact by the Reagan administration budget.

Thank you.

[The prepared statement of Dr. Brinkman follows:]

# UNITED STATES HOUSE OF REPRESENTATIVES

# COMMITTEE ON SCIENCE AND TECHNOLOGY

# SUBCOMMITTEE ON ENERGY RESEARCH AND PRODUCTION

STATEMENT OF

W. F. BRINKMAN

March 2, 1983

I am Bill Brinkman, and I am currently the Director of the Physical Research Laboratory at Bell Laboratories. I am also the Chairman of the Solid-State Sciences Panel of the National Research Council. I am here today to comment on the budget of the Department of Energy for Basic Energy Sciences, Energy Supply Research and Development. My comments and views result not only from my work as a research director at Bell Laboratories, but also as a person who has served on several review committees of the National Laboratories' scientific programs and facilities, on visiting committees of specific laboratories, and on the Solid State Sciences Committee of the National Research Council. I am also Chairman of the new Academy Survey of Physics. Since almost all my experience has been in the area of materials science, I will primarily address issues that I believe are important to this field.

It is currently fashionable to group an enormous breadth of scientific research under the title Materials Research or Materials Science. For example, scientists working in Condensed Matter Physics examining some of the most fundamental questions regarding the collective nature of matter, for which several recent Nobel Prizes have been awarded, are now considered to be part of Materials Science. At the same time, Materials Science includes a large body of scientists interested in the atomic and electronic structure of surfaces and their catalytic activity. It includes scientists working on the growth of new electronic materials by controlled deposition of atomic layers. It includes scientists working on materials to incorporate radioactive wastes for long-term disposal, on metal fatigue and embrittlement, on ceramics, and polymers. Materials scientists spend their time synthesizing new materials and characterizing them with a broad assembly of techniques using X-rays, electron beams, light, and other probes. The underlying motives of researchers in the field are as diverse as the industries that are dependent upon it to provide new or alternative materials - materials for electronic equipment, airplanes and automobiles, the plastics industry and steel industries to name a few. Materials science includes researchers who think of themselves as physicists, chemists, metallurgists, ceramicists, and engineers. The Materials Sciences underlie much of our high and low technology industries and are of unquestionable importance to the long-term health of U.S. industry and technology. Materials science is largely an individual endeavor and the most successful efforts are characterized by their relatively small sizes of 2 to 10 individuals. Again, it is the diversity of individuals and the efforts that have lead to conceptual breakthroughs of significant importance. Thus, it is only natural that a successful materials science effort, in general, is not synonymous with large coherent groups. Priority setting with respect to specifics is a diffused process. The modern day activity of certain fields of research in establishing priorities in their field is most often not a result of the scientists finding that that procedure is the best means of funding research, i.e., that it leads to the most innovative and novel research, but rather is a necessary activity resulting from the fact that research in certain fields has become so expensive that priority setting has become a necessity. Unfortunately, the Materials Science community faces this dilemma today and in the foreseeable future, particularly in the budget of the Department of Energy. While most of materials science does not require large facilities, there are now certain techniques which require facilities costing $10-100 Million. These involve sources of neutrons, ultraviolet light, and X-rays.

Neutron scattering is extremely important in materials science because it represents a unique probe of the properties of materials. It is particularly important as a probe of the magnetic properties of materials but also probes spectroscopic properties of materials that are inaccessable by any other technique. Unfortunately, the only intense sources of neutrons are reactors, two of which are at Brookhaven and Oak Ridge, and the new pulsed sources being explored at Argonne National Laboratory and Los Alamos National Laboratory. These facilities are expensive to run, but are vital to the continued health of materials research in the U.S. Some recent important contributions using neutron scattering have been studies of materials where magnetism and superconductivity have been found to coexist for the first time, important studies of organic conductors - a new class of conducting materials, and studies of polymeric materials - a class of materials where research interest has recently increased.

Another set of major facilities are the synchrotrons at Stanford, Wisconsin, Cornell and Brookhaven. When electrons moving very near the speed of light are forced to change their direction of motion, they emit a broad spectrum of electromagnetic radiation from the far infrared to the X-ray region. In modern machines, this radiation is very intense, particularly in the ultraviolet and X-ray regions, and has allowed whole new classes of experiments to be performed. These include major new studies of surface structure both electronic and atomic which will impact on the nation's energy problems. It also includes important new research in molecular biology and physiology.

The major facilities for this research are very important for the continued health of materials science, not only as it relates to energy research and production but also to the entire materials field with its vital role in supporting the entire industrial enterprise. The important role of the Department of Energy in maintaining and building facilities of this type cannot be underestimated. Indeed, it is one of the ways that the national laboratories make a unique contribution toward the support of American industry. In both neutron studies and in synchrotron radiation research the cooperation between American industry, universities, and the national laboratories, has been outstanding. Scientists from many different industries have performed research at these facilities - IBM, Exxon, Xerox, Monsanto, Bell Laboratories - to name a few. Thus, an important role for the national laboratories in materials science is the support of research facilities that are beyond the means of any individual industry or university. The national laboratories have unique capabilities in technical and engineering talent to perform this role. An important aspect of operating national facilities is the proper balance of users from in-house scientists and outside industry and university participants. We have found that one can error in either direction, and I believe that in the past 10 years, the national laboratories have learned how to maintain a good balance and that our current set of facilities for materials science are highly successful and enjoy continued world-wide recognition.

An important element of this success has been continued interaction with and feedback from the scientific community who will use the facility. This must occur at all stages of planning, construction and operation. The community is in fact rapidly broadening and proper account of its needs and judgements regarding the optimization of our resources is paramount in planning our future facilities. The DOE has an excellent record on this score. It has sought advice by establishing ad hoc committees, by the use of the NAS Solid State Sciences Panel, and by the creation of its own Materials Advisory Council.

While the major national facilities operated by the Department of Energy are vital to our nation's continued success in Materials Science, the majority of materials research is performed by individual scientists or small groups of scientists in their own laboratories. The Department of Energy supports this type of program through its support of University research and through support of a broad spectrum of subject-oriented research programs at the national laboratories. One cannot overemphasize the importance of supporting research which not only generates new knowledge in a field close to our technical industrial base, but also trains scientists and engineers in this area of immediate interest to American Industry. The National Laboratories can and do play a vital role in this activity by having university faculty and students visit and work in their research laboratories. This function is extremely important, particularly where they have unique facilities.

In my opinion, the National Laboratories have two distinct roles to play in supplying unique facilities for the purpose of doing research, assisting universities and interacting with American Industry. I have already discussed one role, that of constructing, operating, and doing research on the large national facilities. The second one is in what some might call medium-sized science vs the usual discussion of big science and little science. Because the national laboratories have strong technical and engineering support for their programs, they have the potential for establishing concentrated efforts toward the development of particular experimental techniques where sophisticated specialized apparatus is necessary. This is a mode that West German research laboratories have adopted, and it has led to the successful development of several new techniques - two examples being low energy electron loss spectroscopy and He atom scattering from surfaces. The important point here is that sophisticated techniques such as these must be developed through the evolutionary construction of apparatus which requires complex engineering and design - each machine often costing several hundred thousand dollars to construct. Such programs are generally outside the capabilities of the research program of individual professors or if established at a particular university would require the university department involved to concentrate its efforts and consequently be too narrow in scope. This is not a desirable direction from the point of view of training students. This middle level of scientific research is not likely to be performed by industry for similar reasons. Such a role for the national laboratories might help to correct the fact that much sophisticated experimental apparatus is now being supplied by European and Japanese suppliers.

An example of a highly successful program that comes close to fitting the prescription of a middle-level scientific program is the effort in ion-solid interactions and laser annealing at Oak Ridge. This program has not only been an excellent scientific program of the Oak Ridge staff, but in addition they have developed a set of "medium-sized apparatus" which is used not only by university collaborators but also by researchers in smaller industries where such facilities do not exist. It has allowed several industries to try ideas they are not equipped to do. This program has fostered both university and industrial interaction.

Finally, I would like to comment on a role the national laboratories should not attempt to play with respect to U.S. industry. While the national laboratories can couple to industry in the fashion I have described above, they cannot and in my opinion should not attempt to get involved in materials development except in those areas in which they have a definite nationally recognized mission. This means that they will not do certain types of materials development such as those involved in the current efforts to develop low-loss optical fibers or materials for semiconductor lasers and detectors. Development work requires an enormous effort that must be closely coupled to the specific requirements and needs of industry. The close coordination of development and commercial needs is often difficult to establish even inside a given company. The national laboratories may miss some research that is a spin-off of development programs but compared to the potential waste in misguided development projects this is unimportant. They can do research on new materials and devices that will ultimately feed into decisions for directions of industrial development programs. Indeed, the laser annealing program at Oak Ridge is of this type.

In order to ensure that programs such as those just described that involve relatively small amounts of money from a national point of view are initiated, it is essential that the climate in Washington should not be one where the only new initiatives that can be supported are those which cost hundreds of millions of dollars. One sometimes finds that it is easier to fund a large program, often with the simplest of goals, than to sell the continuous flexible funding of science where priority setting can be made at a level at which knowledgeable decisions are possible.

In conclusion, I believe the Materials Science program of the Department of Energy is an important part of our national scientific effort in an area that is crucial to the long-term health of U.S. industry. The national laboratories have a crucial role to play in this effort and it is encouraging to see the recognition given to this fact by the Reagan Administration budget.

Mr. **Fuqua**. Thank you very much, Dr. Brinkman.

Dr. Schriesheim, we will be pleased to hear from you at this time.

## STATEMENT OF DR. ALAN SCHRIESHEIM, GENERAL MANAGER, ENGINEERING TECHNOLOGY DEPARTMENT, EXXON RESEARCH & ENGINEERING CO.

Dr. **Schriesheim**. Good morning.

My name is Alan Schriesheim. I am cochairman of the National Research Council Board of Chemical Sciences and Technology. I am a member of the Energy Research Advisory Board, ERAB. I am general manager of Exxon's engineering technology department.

I would like to thank you for the opportunity to present my views on the role of DOE in meeting the needs of the chemical sciences community.

Now, I think the best way to do that is to present highlights of the conclusions and recommendations of the National Research Council Board on Chemical Sciences and Technology's study of basic research in the chemical sciences related to energy.

To give you a little background on the Board, its charter, members, and present methodology, in 1976, the Academy of Sciences Committee on Chemistry and Federal Policy recognized the need for some kind of an oversight committee composed of leaders in the chemistry community.

In 1977, ERDA provided initial funding to set that group up. By 1981, the National Science Foundation, National Institutes of Health, and the Department of Defense were all actively participating, and this board is now functioning as originally planned, as a focal point within the National Research Council for chemistry and chemical technology. We have oversight responsibility for a whole host of varied chemical-related activities such as hazardous substances in the labs, the U.S. National Committee for Pure and Applied Chemistry, and we coordinate our efforts with other NRC groups such as the Solid State Sciences Committee and materials and what not.

Now, the current membership of this board is drawn from academia, industry and Government. It represents a geographic cross section, a chemistry discipline cross-section, and engineering input.

The methodology that we used to develop this report on energy was based on a series of questions: Is this in fact a significant area of energy-related research? Are the principal parts, energy-related parts, included? Has an accurate picture been drawn of the needed kinds of research, recognizing, of course, that energy research, like all such research, involves a judgment call?

We completed this report at the end of 1981, but I think the findings are still pertinent, as are the recommendations. It opened with a transmittal letter from Philip Handler, then the president of the Academy of Sciences. It stressed the inadequacy of basic energy sciences funding. It noted similar conclusions by a series of earlier panels, the Buchsbaum panel of OSTP, the Bennett panel, which was an ERAB panel, and several other panels. I have listed them here in the written testimony.

That means that, over a year ago, five groups had reached the same conclusion, that funding for basic energy research was below that needed to exploit the opportunities that this group visualized in the chemical sciences.

With respect to the technical issues, the study approached the needs from two general directions: basic research with discernible relations to certain energy technologies, and research needs in selected subdisciplines of chemistry. Both of these are important. I don't intend to stagger this group by going into all of them in detail, but certainly one thing we accomplished was to assemble a matrix of chemical research areas against energy technology areas, with an assignment of relative priorities. This really was a very difficult task, as you can appreciate, but a worthwhile effort.

With respect to the funding issue, the basic energy sciences chemical sciences program has not generally kept pace with inflation since 1970. In comparison, the spending for basic research by my own industry, the petroleum industry, has shown a dramatic increase, over 50 percent, since 1972.

The petroleum industry percentages must be qualified because it depends on the way they are reported. The industry in general has consistently increased its percent of R&D funding for basic research since 1972, and indeed, the petroleum industry has traditionally backed up short-range activities with long-range, high-risk research in the hope of establishing major scientific breakthroughs leading to high layout.

Obviously, no one knows with certainty what the right balance between long-range research and short-range developmental activities or between public and private sponsorship of research is. It seems logical, though, that DOE, a Federal agency, should lead the way in fostering research whose potential benefits are large but too general or too long range to attract support from the private sector.

I would like to turn my attention now to some technical aspects of the report. As I mentioned earlier, there are too many to go into in detail. We attempted, really, to identify chemistry-related scientific sticky points: Where is the technology stuck and how can we relate that, then, to some underlying scientific issue that we could then transmit to the basic science community, whether it is DOE, the universities, or the national labs, so that they can get at the so-called scientific sticky point?

We examined approximately 25 chemistry-related areas. We identified the whole plethora of these chemistry-related basic science challenges. Let me give you an example—catalysis. It is a basic science discipline. It is filled with contributions that can be made in physical organic chemistry, surface chemistry, inorganic chemistry, enzyme chemistry. Real breakthroughs are possible, leading to marked lowering of costs, whether it is in conventional processing or nonconventional processing.

As an example, a multidisciplinary attack on the nature of poisoning of catalysts, deactivation and promotion at a molecular level would have a profound influence on the processing of heavy feeds such as coal, shale oil, tar sands, bitumen, and so forth, and the details of that are in the report.

Well, in making funding decisions, whether short or long range, it is vital to consider the impact of the proposal on the technology. Consideration of the impact implies a priority decision, since funds are limited.

To illuminate the impact of such decisionmaking, a matrix was designed to provide perspective for assessing the potential impact of chemistry-focused basic, long-range, and pioneering research in energy technology areas.

By long-range and pioneering research, I mean research that is undertaken to uncover scientific knowledge that will assist broadly in meeting the energy needs over the next several years. It is long range, but it is focused in those areas that we think will reduce the risk of making an impact on energy.

This matrix is certainly not supposed to be used as a blind tool; it is a tool to be used with judgment. Chemistry is a ubiquitous science; characterizing it is an arbitrary exercise. The disciplines and subdisciplines cross over and blend into one another. The number of individual classical disciplines are described in each matrix category. I am really not going to go through the matrix. We split it up into fossil resources, petroleum-related fuels, tar sands, shale, and so forth; other energy sources such as solar, nuclear, and biomass, energy storage, as well as an open category labeled "currently unforeseen," and environmental impact.

For each of the technology sections, then, the matrix lists the potential impact, high or low, of basic long-range and pioneering research.

As an example, in fossil resources, petroleum, agricultural chemistry, is perceived to have a low impact on enhanced oil recovery. So if there are proposals in agricultural research and someone claims that this will have an impact on enhanced oil recovery, we know that that is interesting but probably not accurate.

So we feel that these matrices, when used properly, with judgment, serve to organize thought processes. They clarify funding decisions and they highlight possible areas for academic interest. They display potential strengths and weaknesses in program portfolios. We have transmitted these widely to the scientific engineering community.

Now, I would also like to comment, given the time, in two other areas, within the time constraint, on the major user-related facilities and the national labs versus extramural support.

Certainly the panel was, in the main, strongly supportive of the major user-related facilities of DOE. The Brookhaven National Synchrotron Light Source should have wide and continuing use. Issues here are ancillary funding for guest workers, industry involvement, and so forth, which are among the classical issues that you would list.

These unique Government facilities provide a resource for industrial scientists and an excellent opportunity for scientific interaction and technology transfer. My own company, Exxon, has already spent $1.4 million to build beam lines at the National Synchrotron Light Source at Brookhaven. We plan to spend another $1 million in the near future. We also are spending about $1.5 million on a beam line at Stanford Synchrotron Radiation Lab.

When we add to these costs the technical labor, Exxon alone has a healthy investment in the use of synchrotron radiation using DOE facilities.

The Sandia National Lab's laser diagnostic instrumentation is another user facility that we use. It has good support from academia, the automobile, and petroleum industries. One of our staff members, for example, sits on the board.

We also commented on the underutilization of university researchers. Now, some have interpreted this as advocacy for withdrawal of resources from the national labs to augment university research. That is certainly not our intention. The primary criterion for selection should be quality research, regardless of institutional performer.

We identified with the fact that few outstanding university people are engaged in basic research in areas that bear on synthetic fuels such as coal chemistry. We have started an effort to stimulate leading chemists in a whole host of fields to undertake research in these areas.

We held a workshop in May 1982 which brought together 15 upper echelon research directors from energy companies and chemical companies with major energy research programs, with a number of leading academic chemists. We tried to define how best to organize or stimulate a series of scholarships and visiting professorships to bring academic leaders into close exposure by short visits or longer visits to an active energy research group.

Our own experience at Exxon through a special Exxon fellows program has shown that outstanding scientists certainly can be attracted into such a program, and progress has been made in linking up academic scientists and companies, and the specifics of that are in my statement.

So I would like to summarize, then. There is a rich portfolio of energy-related needs and research opportunities in chemistry and chemical engineering in the energy arena.

The Federal role in basic energy research is important for strong U.S. industry. Access to major facilities is important for the conduct of industrial research programs.

The Academy and the Research Council provide an important function in bringing together representatives of industry, universities, and Government organizations. Planning for large facilities is important, but we should not do that to the exclusion of everything else.

I cannot resist mentioning, a study that is being undertaken by the chemical sciences community under the NRC board, chaired by George Pimentel, who is the director of chemical biodynamics at the University of California and former deputy director at NSF. That study will pinpoint needs and opportunities in this high-tech area which is so important from the standpoint of international competitiveness. I would hope this study would be done sometime this summer, and I am sure this committee will be interested in hearing the results.

Thank you, Mr. Chairman.

[The prepared statement of Dr. Schriesheim follows:]

# TESTIMONY BEFORE THE SUBCOMMITTEE ON ENERGY DEVELOPMENT AND APPLICATIONS

MARCH 2, 1983

A. SCHRIESHEIM

GOOD MORNING. MY NAME IS ALAN SCHRIESHEIM. I AM CO-CHAIRMAN OF THE NATIONAL RESEARCH COUNCIL BOARD OF CHEMICAL SCIENCES AND TECHNOLOGY, A MEMBER OF THE ENERGY RESEARCH ADVISORY BOARD (ERAB) AND GENERAL MANAGER OF THE EXXON ENGINEERING TECHNOLOGY DEPARTMENT.

THANK YOU FOR THE OPPORTUNITY TO PRESENT MY VIEWS ON THE ROLE OF DOE IN MEETING THE NEEDS OF THE CHEMICAL SCIENCES COMMUNITY. I BELIEVE THAT THE BEST WAY TO ACCOMPLISH THIS IS TO PRESENT HIGHLIGHTS OF THE CONCLUSIONS AND RECOMMENDATIONS OF THE NATIONAL RESEARCH COUNCIL BOARD ON CHEMICAL SCIENCES AND TECHNOLOGY'S STUDY OF BASIC RESEARCH IN THE CHEMICAL SCIENCES RELATED TO ENERGY. A SMALL AMOUNT OF BACKGROUND ON THE BOARD, ITS CHARTER, ITS MEMBERS AND THE REPORT METHODOLOGY IS IN ORDER.

IN 1976, THE NATIONAL ACADEMY OF SCIENCES COMMITTEE ON CHEMISTRY AND FEDERAL POLICY RECOGNIZED THE NEED FOR A GOVERNMENT-WIDE OVERVIEW COMMITTEE COMPOSED OF LEADERS OF THE CHEMISTRY RESEARCH COMMUNITY. IN 1977, ERDA PROVIDED FUNDS TO LAUNCH A COMMITTEE ON CHEMICAL SCIENCES AND REQUESTED ADVICE ON RESEARCH SUBDISCIPLINES AND SUPPORT OF FACILITIES UNDER CONSTRAINT OF INCREASED RESPONSIBILITIES BUT STATIC BUDGET.

BY 1981, NSF, NIH & DOD WERE ACTIVELY PARTICIPATING AND WE ARE NOW FUNCTIONING AS ORIGINALLY PLANNED--THE FOCAL POINT WITHIN THE NATIONAL RESEARCH COUNCIL FOR CHEMISTRY AND CHEMICAL TECHNOLOGY. WE HAVE OVERSIGHT RESPONSIBILITY FOR VARIED CHEMICAL-RELATED ACTIVITIES SUCH AS HAZARDOUS SUBSTANCE IN THE LABS AND THE UNITED STATES NATIONAL COMMITTEE FOR PURE AND APPLIED CHEMISTRY. WE ARE ALSO CHARGED WITH COORDINATING OUR EFFORT WITH OTHER NRC GROUPS SUCH AS THE SOLID STATE SCIENCES COMMITTEE.

THE CURRENT MEMBERSHIP OF THE BOARD ON CHEMICAL SCIENCES AND TECHNOLOGY IS DRAWN FROM ACADEMIA, INDUSTRY AND THE GOVERNMENT. IT REPRESENTS A GEOGRAPHIC CROSS-SECTION, A CHEMISTRY DISCIPLINE CROSS-SECTION AND CHEMICAL ENGINEERING INPUT.

LET ME DESCRIBE THE METHODOLOGY USED IN DEVELOPING THE REPORT ON ENERGY. WE RELIED ON THE CONSIDERABLE BREADTH OF PERSPECTIVE REPRESENTED BY OUR MEMBERSHIP TO IDENTIFY PROBABLE AREAS FOR EMPHASIS. PRELIMARY AREAS WERE EACH ASSIGNED TO A SUBCOMMITTEE, WITH THE INSTRUCTION TO EXAMINE THE NEEDS, WITH HELP, BY CORRESPONDENCE, FROM ADDITIONAL EXPERTS. AT OUR BIMONTHLY MEETINGS, THE SUBCOMMITTEES PRESENTED INITIAL DRAFTS FOR DISCUSSION BY THE FULL COMMITTEE. THE KINDS OF QUESTIONS WEIGHED WERE: (a) IS THIS IN FACT A SIGNIFICANT AREA OF ENERGY-RELATED RESEARCH? (b) ARE ITS PRINCIPAL ENERGY-RELATED PARTS INCLUDED? (c) HAS AN ACCURATE PICTURE BEEN DRAWN OF THE NEEDED KINDS OF RESEARCH-RECOGNIZING, OF COURSE, THAT THIS INVOLVES A JUDGMENT CALL? SEVERAL ITERATIONS OF THIS KIND WERE MADE FOR EACH AREA. TOWARD THE END, OUR SUMMARY SECTIONS WERE SIMILARLY

DEVELOPED. AND WE ALSO CONSIDERED HOW BEST TO DISSEMINATE OUR REPORT TO STIMULATE FULLEST CONSIDERATION AND IMPLEMENTATION.

ALTHOUGH COMPLETED IN 1981, THE FINDINGS ARE STILL PERTINENT AS ARE THE RECOMMENDATIONS.

THE REPORT WAS COMPREHENSIVE, COVERING A WIDE VARIETY OF SCIENCE/POLICY ISSUES.

IT OPENS WITH A TRANSMITTAL LETTER FROM PHILLIP HANDLER, THEN THE PRESIDENT OF THE NATIONAL ACADEMY OF SCIENCES, STRESSING INADEQUACY OF BES FUNDING AND NOTING SIMILAR CONCLUSIONS BY EARLIER GROUPS

- BUCHSBAUM PANEL - OSTP
- BENNETT PANEL - ERAB - 1980
- CCS NRC 1979 REPORT
- ERAB R&D PANEL - NOVEMBER 1981

OVER A YEAR AGO, FIVE GROUPS HAD REACHED THE SAME CONCLUSION. THE FUNDING FOR BASIC RESEARCH WAS BELOW THAT NEEDED TO EXPLOIT THE OPPORTUNITIES IN CHEMICAL SCIENCES.

WITH RESPECT TO THE TECHNICAL ISSUES, THE STUDY APPROACHED NEEDS FROM TWO GENERAL DIRECTIONS: BASIC RESEARCH WITH DISCERNIBLE RELATIONS TO CERTAIN ENERGY TECHNOLOGIES, AND RESEARCH NEEDS IN SELECTED SUBDISCIPLINES OF CHEMISTRY. BOTH ARE IMPORTANT AND I WILL HIGHLIGHT SOME EXAMPLES OF EACH SHORTLY.

ONE THING WE ACCOMPLISHED WAS TO ASSEMBLE A MATRIX OF CHEMICAL RESEARCH AREAS AGAINST ENERGY TECHNOLOGY AREAS, WITH ASSIGNMENT OF RELATIVE PRIORITIES. THIS IS A DIFFICULT TASK BUT A WORTHWHILE EFFORT. MORE ON THIS LATER.

WITH RESPECT TO THE FUNDING ISSUE, THE BASIC ENERGY SCIENCES CHEMICAL SCIENCES PROGRAM HAS GENERALLY NOT KEPT PACE WITH INFLATION SINCE 1970. FOR COMPARISON, THE SPENDING FOR BASIC RESEARCH BY THE PETROLEUM INDUSTRY HAS SHOWN A DRAMATIC INCREASE -- OVER 50 PERCENT -- SINCE 1972. WHILE PETROLEUM INDUSTRY PERCENTAGES MUST BE QUALIFIED OWING TO THE METHOD OF REPORTING, THE INDUSTRY HAS CONSISTENTLY INCREASED ITS PERCENTAGE OF R&D FUNDING FOR BASIC RESEARCH SINCE 1972.

INDEED, THE PETROLEUM INDUSTRY HAS TRADITIONALLY BACKED UP SHORT RANGE ACTIVITIES WITH LONG RANGE, HIGH RISK RESEARCH IN THE HOPE OF ESTABLISHING MAJOR SCIENTIFIC BREAKTHROUGHS LEADING TO HIGH PAYOUT.

NO ONE KNOWS WITH CERTAINTY WHAT IS THE RIGHT BALANCE BETWEEN LONG RANGE BASIC RESEARCH AND SHORT RANGE DEVELOPMENT ACTIVITIES, OR BETWEEN PUBLIC AND PRIVATE SPONSORSHIP OF RESEARCH. HOWEVER, IT SEEMS LOGICAL THAT THE DOE, A FEDERAL AGENCY, SHOULD LEAD THE WAY IN FOSTERING RESEARCH WHOSE POTENTIAL

BENEFITS ARE LARGE, BUT TOO GENERAL OR TOO LONG RANGE TO ATTRACT SUPPORT FROM THE PRIVATE SECTOR.

NOW I WOULD LIKE TO TURN MY ATTENTION TO SOME TECHNICAL ASPECTS OF THE REPORT. THERE ARE SIMPLY TOO MANY DETAILED TECHNICAL RECOMMENDATIONS TO DISCUSS IN THE ALLOTTED TIME. IN GENERAL, WE ATTEMPTED TO IDENTIFY CHEMISTRY-RELATED SCIENTIFIC "STICKY POINTS".

THESE ARE "POINTS" WHERE THE TECHNOLOGY IS STUCK WHICH REQUIRES CHEMISTRY-RELATED BASIC SCIENCE TO MOVE FORWARD. WE EXAMINED APPROXIMATELY 25 CHEMISTRY-RELATED AREAS AND IDENTIFIED A PLETHORA OF CHEMISTRY-RELATED BASIC SCIENCE CHALLENGES.

FOR EXAMPLE, CATALYSIS IS A BASIC SCIENCE DISCIPLINE FIELD WITH CONTRIBUTIONS TO BE MADE IN PHYSICAL-ORGANIC CHEMISTRY, SURFACE CHEMISTRY, INORGANIC CHEMISTRY, ENZYME CHEMISTRY AND SO ON. REAL BREAKTHROUGHS ARE POSSIBLE LEADING TO MARKED LOWERING OF COSTS. FOR EXAMPLE, A MULTI-DISCIPLINARY ATTACK ON THE NATURE OF POISONING, DEACTIVATION AND PROMOTION AT THE MOLECULAR LEVEL WOULD HAVE PROFOUND INFLUENCE ON THE PROCESSING OF HEAVY FEEDS SUCH AS COAL, SHALE OIL AND TAR SANDS BITUMEN.

IN MAKING FUNDING DECISIONS, WHETHER SHORT OR LONG RANGE, IT IS VITAL TO CONSIDER THE IMPACT OF THE PROPOSAL ON TECHNOLOGY. CONSIDERATION OF SUCH IMPACT IMPLIES A PRIORITY DECISION, SINCE FUNDS ARE LIMITED. TO ILLUMINATE THE IMPACT OF SUCH DECISION-MAKING, A MATRIX WAS DESIGNED TO PROVIDE PERSPECTIVE FOR ASSESSING THE POTENTIAL IMPACT OF CHEMISTRY-FOCUSED BASIC, LONG-RANGE, AND PIONEERING RESEARCH ON ENERGY TECHNOLOGY AREAS. BY LONG RANGE AND PIONEERING RESEARCH IS MEANT RESEARCH UNDERTAKEN TO UNCOVER SCIENTIFIC KNOWLEDGE THAT WILL ASSIST BROADLY IN MEETING ENERGY NEEDS OVER THE NEXT SEVERAL DECADES.

CHEMISTRY IS A UBIQUITOUS SCIENCE, AND CATEGORIZING IT IS AN ARBITRARY EXERCISE. CHEMISTRY DISCIPLINES AND SUB-DISCIPLINES CROSS OVER AND BLEND INTO ONE ANOTHER, AND A NUMBER OF INDIVIDUAL CLASSICAL DISCIPLINES ARE INCLUDED IN EACH MATRIX CATEGORY, AS DESCRIBED BELOW. FOR CONVENIENCE, THE MATRIX IS DIVIDED INTO THREE TECHNOLOGY SECTIONS, NAMELY:

I. FOSSIL RESOURCES -- PETROLEUM AND RELATED FUELS - TAR SANDS AND SHALE, COAL AND PETROCHEMICALS.

II. OTHER ENERGY RESOURCES -- SOLAR, NUCLEAR, BIOMASS, ENERGY STORAGE, ETC., AS WELL AS AN OPEN CATEGORY LABELED "CURRENTLY UNFORESEEN."

III. ENVIRONMENTAL IMPACT

FOR EACH OF THE TECHNOLOGY SECTIONS, THE MATRIX LISTS THE POTENTIAL IMPACT -- HIGH OR LOW -- OR BASIC, LONG RANGE AND PIONEERING RESEARCH.

A PORTION OF SECTION 1 - FOSSIL RESOURCES PETROLEUM ILLUSTRATES THE POINT. AGRICULTURAL CHEMISTRY FOR EXAMPLE IS

PERCEIVED TO HAVE A LOW IMPACT ON ENHANCED OIL RECOVERY.

WE FEEL THAT THESE MATRICES USED PROPERLY I.E. WITH JUDGEMENT SERVE TO ORGANIZE THOUGHT PROCESSES, CLARIFY FUNDING DECISIONS, HIGHLIGHT POSSIBLE AREAS FOR ACADEMIC INTEREST AND DISPLAY POTENTIAL STRENGTHS AND WEAKNESSES IN PROGRAM PORTFOLIOS.

THE REPORT ALSO COMMENTED ON A VARIETY OF OTHER ISSUES. AGAIN, WITHIN THE TIME CONSTRAINT, TWO ARE HIGHLIGHTED:

- MAJOR USER RELATED FACILITIES
- NATIONAL LABS VS. EXTRAMURAL SUPPORT

OUR PANEL WAS IN THE MAIN STRONGLY SUPPORTIVE OF THE MAJOR USER RELATED FACILITIES OF DOE. FACILITIES SUCH AS THE BROOKHAVEN NATIONAL SYNCHOTRON LIGHT SERVICE SHOULD HAVE WIDE AND CONTINUOUS USE. ISSUES HERE, OF COURSE, ARE ANCILLARY FUNDING FOR GUEST WORKERS, INDUSTRY INVOLVEMENT, ETC.

UNIQUE GOVERNMENT FACILITIES PROVIDE A RESOURCE FOR INDUSTRIAL SCIENTISTS AND AN EXCELLENT OPPORTUNITY FOR SCIENTIFIC INTERACTION AND TECHNOLOGY TRANSFER. FOR EXAMPLE, EXXON HAS ALREADY SPENT $1.4M TO BUILD BEAM LINES AT THE NATIONAL SYNCHROTRON LIGHT SOURCE AT BROOKHAVEN NATIONAL LABORATORY AND PLANS TO SPEND ANOTHER $1.0M IN THE NEAR FUTURE.

WE ARE ALSO SPENDING $1.4M ON A BEAM LINE AT STANFORD SYNCHROTRON RADIATION LABORATORY. WHEN WE ADD TO THESE COSTS THE TECHNICAL LABOR, EXXON ALONE HAS A HEALTHY INVESTMENT IN THE USE OF SYNCHROTRON RADIATION USING DOE FACILITIES.

ANOTHER FACILITY IN THE BES PICTURE WHICH IS A UNIQUE RESOURCE FOR THE RESEARCH COMMUNITY IS THE COMBUSTION RESEARCH FACILITY AT SANDIA NATIONAL LABORATORY, LIVERMORE. IT PROVIDES THE MOST ADVANCED LASER DIAGNOSTIC INSTRUMENTATION AVAILABLE FOR THE STUDY OF COMBUSTION PROCESSES IN FLAMES, BURNERS AND ENGINES. THIS IS A USER FACILITY, SUPPORTED BY THE DIVISION OF CHEMICAL SCIENCES UNDER THE BASIC ENERGY SCIENCES PROGRAM WHICH IS ACCESSIBLE TO SCIENTISTS FROM INDUSTRY, ACADEMIA AND GOVERNMENT LABORATORIES. IT HAS THE SUPPORT OF THE AUTOMOBILE AND PETROLEUM INDUSTRIES, MEMBERS OF WHICH SIT ON THEIR ADVISORY COMMITTEE.

IN ITS RECENT REPORT, THE NRC PANEL ALSO COMMENTED ON THE UNDERUTILIZATION OF UNIVERSITY RESEARCHERS. SOME HAVE INTERPRETED THIS AS ADVOCACY FOR WITHDRAWAL OF RESOURCES FROM THE NATIONAL LABORATORIES TO AUGMENT UNIVERSITY RESEARCH; THAT IS NOT OUR INTENTION. THE PRIMARY CRITERION FOR SELECTION SHOULD BE QUALITY RESEARCH -- REGARDLESS OF INSTITUTIONAL PERFORMER.

THE COMMITTEE NOTED THE RELATIVELY FEW OUTSTANDING CHEMISTS ENGAGED IN BASIC RESEARCH AREAS BEARING STRONGLY ON SYNTHETIC FUELS, SUCH AS COAL CHEMISTRY. AND, WE HAVE STARTED AN EFFORT TO STIMULATE LEADING CHEMISTS -- IN FIELDS RANGING FROM SYNTHETIC AND NATURAL PRODUCT ORGANIC CHEMISTRY TO CHEMICAL

PHYSICS -- TO UNDERTAKE BASIC, LONG RANGE CHEMISTRY RESEARCH POTENTIALLY PROMISING FOR IMPROVED TECHNOLOGIES FOR CONVERSION OF COAL TO GASEOUS AND LIQUID SYNTHETIC FUELS, OR FEEDSTOCKS FOR CHEMICAL INDUSTRY.

IN MAY 1982, A WORKSHOP WAS HELD WHICH BROUGHT TOGETHER 15 UPPER ECHELON RESEARCH DIRECTORS FROM ENERGY COMPANIES AND FROM CHEMICAL COMPANIES WITH MAJOR ENERGY RESEARCH PROGRAMS, WITH A COMPARABLE NUMBER OF LEADING ACADEMIC CHEMISTS. THE WORKSHOP TRIED TO DEFINE HOW BEST TO ORGANIZE (OR STIMULATE) A PROGRAM OF FELLOWSHIPS AND VISITING PROFESSORSHIPS TO BRING ACADEMIC LEADERS INTO CLOSE EXPOSURE, PERHAPS BY A SERIES OF SHORT VISITS OVER A PERIOD OF MONTHS, OR BY A ONE-TIME LONGER VISIT TO AN ACTIVE ENERGY RESEARCH GROUP. OUR OWN EXPERIENCE AT EXXON -- (THROUGH A SPECIAL EXXON FELLOWS PROGRAM) -- HAS SHOWN THAT OUTSTANDING SCIENTISTS, SUCH AS DUDLEY HERSCHBACH OF HARVARD AND JOHN SCHRIEFFER OF U.C. - SANTA BARBARA, CAN BE ATTRACTED TO AN EXTENDED STAY AT THE COMPANY'S LABORATORY. AND THAT SUCH AN INTERACTION STIMULATES THE SCIENTISTS INTEREST AND RESEARCH ACTIVITY IN NEW FIELDS.

PROGRESS HAS BEEN MADE; A LINKUP HAS BEEN ESTABLISHED BETWEEN AN ACADEMIC SCIENTIST AND ONE COMPANY. ANOTHER COMPANY HAS INDICATED THAT IT WILL INITIATE ADDITIONAL ACADEMIC CONTACTS. AND TWO OTHER COMPANIES ARE EXCITED ABOUT PURSUING ADDITIONAL ACADEMIC/COMPANY CONTACTS IN MORE DEPTH. AN ACADEMIC PARTICIPANT PROPOSED INVITING AN INDUSTRIAL ENERGY EXPERT TO SPEND TIME IN ACADEMICS TO INFORM THEM OF ENERGY RELATED PROBLEMS.

TO SUMMARIZE:

- THERE IS A RICH PORTFOLIO OF ENERGY RELATED NEEDS AND RESEARCH OPPORTUNITIES IN CHEMISTRY AND CHEMICAL ENGINEERING.
- THE FEDERAL ROLE IN BASIC ENERGY RESEARCH IS IMPORTANT FOR A STRONG U.S. INDUSTRY.
- ACCESS TO MAJOR FACILITIES IS IMPORTANT FOR THE CONDUCT OF INDUSTRIAL RESEARCH PROGRAMS.
- THE NAS/NRC PROVIDES IMPORTANT FUNCTIONS IN BRINGING TOGETHER REPRESENTATIVES OF INDUSTRIAL, UNIVERSITY AND GOVERNMENT ORGANIZATIONS.
- PLANNING FOR LARGE FACILITIES IS IMPORTANT BUT SHOULD NOT BE DONE TO THE EXCLUSION OF EVERYTHING ELSE.

IN CLOSING, I CAN'T RESIST MENTIONING A STUDY BEING UNDERTAKEN BY THE CHEMICAL SCIENCES COMMUNITY UNDER THE AEGIS OF THE NRC BOARD ON CHEMICAL SCIENCES AND TECHNOLOGY CHAIRED BY GEORGE PIMENTEL, DIRECTOR OF THE LABORATORY OF CHEMICAL BIO DYNAMICS AT THE UNIVERSITY OF CALIFORNIA, BERKELEY, AND FORMER DEPUTY DIRECTOR OF NSF. THIS STUDY WILL PINPOINT NEEDS AND OPPORTUNITIES IN THE HIGH TECH AREA WHICH IS SO IMPORTANT FROM THE STANDPOINT OF INTERNATIONAL COMPETITIVENESS. FOR EXAMPLE, THE EXCITING NEW DEVELOPMENTS IN BIOTECHNOLOGY ARE VERY MUCH RELATED TO GAINING CONTROL OVER REACTIONS - THAT IS INCREASING SELECTIVITY - THIS HAS STRONG IMPLICATIONS FOR CATALYSIS - A CRITICAL ELEMENT IN THE ENERGY SECTOR. ADVANCES IN CHEMISTRY ARE CRUCIAL TO SUCH REACTION CONTROL. I ANTICIPATE THAT THE STUDY, WHEN COMPLETED LATE THIS YEAR, WILL SURFACE A PORTFOLIO OF EXCITING OPPORTUNITIES IN CHEMISTRY RELATED AREAS.

Mr. **Fuqua**. Thank you very much.
Dr. Rosenzweig.

## STATEMENT OF DR. ROBERT M. ROSENZWEIG, PRESIDENT, ASSOCIATION OF AMERICAN UNIVERSITIES

Dr. **Rosenzweig**. Thank you, Mr. Chairman.

My name is Robert Rosenzweig, and I am president of the Association of American Universities. I have only recently come to Washington and assumed this position, so this is my first appearance before you.

Before coming here, I was at Stanford University, and in that capacity I suppose I spent as much time in Mr. Mineta's office as I did with any other Member of the Congress, even though Stanford is a neighbor and not a constituent of Mr. Mineta's. However, he always saw very clearly that the health of the university was enormously important to the health of many high-technology companies that were his constituents. So I guess you could say we adopted one another as Congressman and constituent by proxy. So it is a special pleasure, an added bonus, to appear before him this morning.

I have a statement for the record, Mr. Chairman. If I may, I will just go through and highlight some of the main points.

Mr. **Fuqua**. Yes. We will make your statement part of the record if you wish to summarize.

Dr. **Rosenzweig**. Thank you.

I will first give our general reaction to the Department's proposals and budget requests for university programs and then try to place those in the context of the present condition and the needs of the Nation's research and graduate education enterprise.

In recent years, it seems to us we have seen a substantial alteration in the research-related mission of the Department. Research programs formerly conducted on university campuses have been either reduced or terminated, and while we might agree that a shift in the mix between basic and applied research was appropriate, there is some concern—I am not sure I would put it higher than the level of a concern, but certainly it is at least a concern—within the university community that the pendulum may have swung too far in certain areas, driving out needed and appropriate university research and training efforts in areas, for example, such as fossil fuels.

This is something we hope the committee will watch carefully to insure that valuable research and education opportunities are not lost and that we continue to train in appropriate ways the next generation of engineers, scientists, and technologists who will work in these areas.

A word about the ongoing research programs: There are several points to be made. First, with regard to the basic energy sciences program, we are pleased to see that a substantial increase is requested for the seven subprograms that are constituents of that major program. The budget requests an increase of $67.3 million, 24 percent, for them. This is a vigorous commitment to basic science.

I am certainly not qualified to speak to the scientific and technical details of the various budgeted activities. I happen to be a political scientist myself, not a real scientist. I would make the following observation: Most of the proposed increases are targeted on new initiatives to be carried out in special national centers. A relatively small part of the overall increase appears to us to be devoted to strengthening the ongoing core university research programs.

The budget provides for only modest increases in most subprogram areas, and it seems these, too, are carefully targeted on those that appear to have the greatest promise of short-term payoff.

Without questioning the merits of any of the proposed new initiatives, I do raise for your consideration the question of whether sufficient attention is being given to maintaining the vitality of ongoing DOE core university research programs and to the unique contributions and the required national investment in those programs carried out in the university laboratories.

Now, research, Mr. Chairman, takes place in an institutional setting. I want to offer a few observations about the condition of the setting in universities today.

The quality of the environment for academic science and graduate education has suffered prolonged erosion. Today many of our leading graduate schools are seeing graduate enrollments tumble as undergraduate students clamor for the limited spaces available in our schools of engineering and computer sciences.

The quality of instruction is being threatened. Faculty are strained to meet the demand, often without adequate assistance. More worrisome yet is the awareness that the ranks of truly talented young faculty coming along behind them are thinning. Time to pursue research frequently is squeezed, and the questions researchers are able to pursue in their investigations often are circumscribed by the obsolete instruments available to them.

The capacity of faculty to substitute ingenuity for modern research tools is diminishing. Chewing gum and string and baling wire do not go as far now as they did 25 or 30 years ago.

In addition, many laboratories are aging or aged. They require modernization if they are to serve through the decade as effective sites for the conduct of modern research.

Now, the Federal Government remains today the most substantial patron of science. However, its investment in core programs designed to strengthen graduate education and contribute to the research and education environment in universities has diminished almost to the vanishing point in crucial areas. Four of those areas need attention now.

First is graduate education. The number of federally funded fellowships has declined precipitously—I do not think that is an exaggeration—from 51,000 in 1968 to fewer than 10,000 today. Of these, only about 1,500 are awarded to students pursuing advanced study in the sciences and engineering, only about 25 of which are awarded by the Department of Energy to students in one or two carefully selected areas such as nuclear science.

This committee has taken the initiative in the past in fashioning a responsible commitment by DOE to the needs of graduate students in energy-related fields. We believe that attempt is worth the effort, and we urge the committee again to consider the need for a DOE graduate fellowship program.

Second and closely linked to the needs of graduate students are the increasingly serious challenges that face young researchers who seek to begin their careers in basic science.

The National Science Foundation has proposed a new Presidential young investigators awards program that would provide about 200 recent Ph. D.'s with 5-year awards averaging $30,000, to be matched from private sources. These funds would enable these potential new researchers to undertake a research program promptly and to have a genuine opportunity to develop their skills as investigators in their early years.

Just as the Department of Energy has followed the lead of NSF and the Department of Defense by establishing a new university research equipment program, so we believe it should give serious consideration to undertaking a comparable program targeted on enhancing the human resources pool in energy-related fields. We suggest that the committee consider adding such an initiative to the fiscal year 1984 DOE authorization.

The third area requiring attention is the need to address the serious problems posed for modern research and quality graduate education by obsolete research tools. Investment in research instrumention by some Federal agencies has increased significantly. The fiscal year 1984 budget request for NSF, for example, proposes a significant increase in support for equipment within its research program.

The Department of Defense has recognized the consequences to its basic research effort of leaving the problem unaddressed, and DOD this year is proposing the second year of a 5-year, $150 million program to update research instrumentation in laboratories to carry out research programs of interest to the Department.

We are pleased to note that the Department of Energy also has joined in this effort. The fiscal year 1984 budget request includes $6 million for a new program to begin to address the equipment needs of laboratories in which energy-related research is done. We urge the committee to fully support this important initiative as a starting point toward a sustained reinvestment strategy for the decade of the 1980's.

We believe the program should grow quickly to a level roughly equal to that of the DOD program—that is, $30 million a year—and be sustained for at least 5 years at that level. We ask the committee to consider that level as an appropriate and necessary objective for this new initiative.

Finally, the need to address the problems associated with deteriorated research laboratories: The Department, in its budget request, states the general problem well, at least as it affects certain DOE facilities. The Department has requested $40 million for the multiprogram general purpose facilities, and it stated in its justication:

> The program is based on the premise that through continuous usage and aging, facilities deteriorate over time to a point where they are no longer useful to perform their intended functions and must be rehabilitated or replaced.

In facing the serious problems of the DOE multiprogram laboratories, the Department is addressing the same problems spawned by obsolete facilities faced by many university researchers who seek to carry out DOE research programs in outdated laboratory buildings. The need to replace, renovate, rehabilitate, and reequip old research laboratories is the other half of the research equipment problem.

In many ways, the equipment and facilities problems are intertwined. For more than 15 years, Mr. Chairman, the Federal Government has invested essentially no funds to assist institutions in the replacement and repair of those laboratories in which the Nation's basic science programs are conducted.

We believe it is time for the leading agencies of the Federal Government that fund the Nation's basic science programs to assist in the rehabilitation of those laboratories essential to each agency's research mission and goals.

In the case of the Department of Energy, we ask the committee to consider establishing a program to rehabilitate DOE-funded university laboratories at a level equal to the multiprogram general purpose facilities program; that is $40 million.

The fiscal year 1984 budget request by the Department of Energy sustains and strengthens the Department's commitment to university research programs. We ask the committee to support fully the Department's requests and to build upon those proposals along the lines we have suggested here.

Thank you, Mr. Chairman.

[The prepared statement of Dr. Rosenzweig follows:]

# STATEMENT

on the
FY 1984
AUTHORIZATION
for the

DEPARTMENT OF ENERGY

before the
Subcommittee on Energy Development & Applications
of the
Committee on Science and Technology

U.S. House of Representatives

by

Robert M. Rosenzweig
President, Association of American Universities

on behalf of the
American Council on Education
and
National Association of State Universities
and Land-Grant Colleges

March 2, 1983

# Testimony before the Subcommittee on Energy Development & Applications of the House Committee on Science & Technology

Mr. Chairman and Members of the Committee:

My name is Robert Rosenzweig, and I am President of the Association of American Universities. Since I have only recently assumed this position, this is my first appearance before you. I represent the 50 universities which comprise our membership, plus the following higher education associations: the American Council on Education, the Association of Graduate Schools, the Council of Graduate Schools in the United States, and the National Association of State Universities and Land-Grant Colleges. As this Committee knows well, these associations together represent essentially all of the nation's universities and colleges which carry out research and related education programs supported by the Department of Energy. My assignment this morning is to present to you our views with regard to the FY 1984 budget request for DOE university research and related programs. In doing so, I will first give our general reaction to the Department's proposals and budget requests for university programs, and then place these in the context of the present condition and needs of the nation's research and graduate education enterprise.

## GENERAL COMMENTS

In recent years we have witnessed a substantial alteration in the research-related mission of the Department. Many research programs formerly conducted on university campuses have been either reduced or terminated. While we might agree that a shift in the mix between basic and applied work is needed, there is some concern within the university community that the pendulum may have swung too far in certain areas, driving out needed and appropriate university research and training efforts in such areas, for example, as fossil fuels. This is something I hope the Committee will watch carefully to ensure that valuable research and education opportunities are not lost and that we do continue to train in appropriate ways the next generation of engineers, scientists, and technologists who will work in these areas.

Turning to the ongoing research programs, there are several points to be made. First, with regard to the Basic Energy Sciences programs, we are pleased indeed to see that a substantial increase is requested for the seven subprograms which comprise the BES program. The budget requests an increase of $67.3 million (24%) for them. This represents a vigorous commitment to basic science. We are not qualified to speak to the scientific and technical details of the various budgeted activities, but I would make the following observation. Most of the proposed increases are targeted on new initiatives to be carried out in special national centers; a relatively small part of the overall increase appears to be devoted to strengthening the ongoing core university research programs. The budget provides for only "modest increases" in most subprogram areas, and it seems these too are carefully targeted on those that appear to have the greatest promise of short-term payoff. Without questioning the merits of the proposed new initiatives, I do raise for your consideration the question of whether sufficient attention is being given to maintaining the vitality of the ongoing DOE core university research programs, to their unique contributions, and to the required national investment in the programs carried out in the laboratories of our universities.

## THE CONTEXT

A reasoned answer to these questions requires an appreciation for the present condition of our research and advanced education capabilities and the place of quality programs of graduate education in our complicated world. In a nation that seems to grow more complex and risky at every turn, it is to our graduate schools that we turn for the continuing supply of talented individuals with the knowledge, technical skills, and perception on which depend our national security, our economic vitality, and cultural well-being. Without the well-educated men and women produced by our graduate and professional schools, we would be unable to sustain our economy, ensure our defense, or hold our position of international leadership.

Assuring a continuing infusion into our society of such talent is clearly a vital national interest. It stands as the major purpose and justification for the federal government's continuing attention to the health of graduate education, and to the need for sustained investment in academic science.

A simple and clear prescription can serve as a guide to national policy with respect to graduate education. It is this: attend to the education and training of the nation's best young minds or fall behind those nations that do.

But for a decade or more, as a nation we have departed from this prescription, and now the costs to us are beginning to be counted. Successive national administrations of both parties, confronted with real economic problems and the need to reduce federal expenditures, have yielded too easily to the half-truth and the seductive notion that freely operating labor markets, unaided by external stimulus or correction, will produce the optimum number of highly trained, first-class people, distributed as needed throughout society.

Now, we see differently. There is ample evidence that the nation cannot presume the availability of a sufficient cadre of highly trained and talented scientists and scholars unless national policy provides incentives adequate to bring about that result. For fifteen years, federal support for graduate education has been reduced substantially. The national level of excellence in graduate education, especially in the context of sharply increasing international competition, has declined to the point that critical national needs served by graduate education are going unmet.

The quality of the environment for academic science and graduate education has suffered prolonged erosion. Today many of our leading graduate schools are seeing graduate enrollments tumble as undergraduate students clamor for the limited spaces available in our schools of engineering and computer science. The quality of instruction is being threatened. Faculty are strained to meet the demand, often without adequate assistance. More worrisome is the awareness that the ranks of truly talented young faculty coming along behind them are thinning. Time to pursue research frequently is squeezed, and the questions researchers are able to pursue in their investigations often are circumscribed by the obsolete instruments available to them. The capacity of faculty to substitute ingenuity for modern research tools is diminishing in many laboratories. In addition, many laboratories are aged; they require modernization if they are to serve through the decade as effective sites for the conduct of modern research. But, despite all of this, it is true that excellent research and quality education programs do continue in many laboratories. Many of those closest to the scene know, however, that increasingly serious problems do plague our research enterprise, problems that left unaddressed will compromise our future.

Essentially all of our national policy objectives depend heavily on graduate education, both for highly trained personnel and for advances in scientific and technical knowledge that sustain our economy and ensure our defense. The present Administration has charted a course that requires significant real increases in total support for research and development, most notably in the area of national defense. The Department of Defense has proposed to increase its investment in university research programs, to initiate a small, new program of graduate fellowships designed to attract a few of the most able undergraduates into graduate programs in fields linked to our national security needs, and it is undertaking a new, 5-year, $150 million program to upgrade research instrumentation in laboratories that carry out research programs in areas related to our defense needs.

The FY 1984 budget request for the National Science Foundation provides significant real increases in the Foundation's physical sciences and engineering programs. After almost a decade of neglect, steps to strengthen the NSF graduate fellowship programs are proposed. Some targeted attention also is being proposed to assist young researchers, who, with some encouragement at a crucial point in their careers, in pursuing careers in academic science. But as welcome as these steps are, they are only the first steps toward a more comprehensive, governmentwide investment strategy for strengthening the underpinnings for basic science and excellence in graduate education.

As we consider our future, it always is good to reflect on the nation's history. It was the sudden dependence on university-based science and scientists during World War II that led to a major emphasis on federal support for university programs of research and graduate training. The successful launching of Sputnik produced a new surge of federal interest in research and advanced education as components of a national effort to regain and sustain American scientific and technological superiority.

Beginning in the 1950s, by a variety of direct and indirect means, the federal commitment to excellence in higher education and research was developed. A tapestry of federal and private programs contributed to strengthening our research and graduate enterprise. At its peak, the commitment to graduate education included not only the National Science Foundation but the major mission agencies as well. The contributions by the Department of Defense and NASA to the health of graduate education are well known to the Congress and to this Committee, in particular. The leadership provided by this Committee made many of these earlier investments possible. But although the federal government remains today a substantial patron for science, its investment in core programs designed to strengthen graduate education and contribute to the research and education environment in universities has diminished almost to the vanishing point in crucial areas. Four areas appropriate for consideration in the context of the FY 1984 DOE budget request are: graduate student support, the need to enhance career prospects for young faculty, research instrumentation, and research facilities needs.

## Graduate Education

The existing matrix of federal programs providing graduate student support is the remnant of a clear past recognition by the federal government of the importance to the nation of strong graduate education programs. Many talented students have pursued advanced education in a wide range of academic specialties simply because federal assistance in many forms made it possible.

Federally funded fellowships were a clear success and an important, even determining, factor in the careers of numerous researchers who now are faculty in our leading institutions. The number of federally funded fellowships has declined precipitously, from 51,000 in 1968 to fewer than 10,000 today; of these only about 1,500 are awarded to students pursuing advanced study in the sciences and engineering, only about 25 of which are awarded by the DOE to students working in one or two carefully selected areas, such as nuclear science.

This Committee has taken the initiative in the past in fashioning a responsible commitment by DOE to the needs of graduate students in energy-related fields. We believe that attempt was worth the effort, and we urge the Committee again to consider the need for a DOE graduate fellowship program. The new graduate fellowship program implemented this year by the Office of Naval Research offers a good model. An even stronger, more balanced approach would provide for a modest program, funded steady-state at perhaps $5 million, which would provide for merit-based, portable, 3-year graduate awards to individuals and also for an equal number of awards made to students through institutions that carry out DOE-funded basic research programs. Such balance between proven approaches is mutually reinforcing;

and, for a relatively small investment by DOE, it would have a substantial beneficial impact on the health of science and engineering departments that carry out energy-related research.

## YOUNG FACULTY

Closely linked to the needs of graduate students are the increasingly serious challenges that face young researchers who seek to begin careers in basic science. As young Ph.D.s complete their formal training, those who aspire to academic careers face hurdles more serious than those their faculty mentors encountered. The attractiveness of an academic career is diminished and the prospects for a successful start need enhancing.

The 1982 AAU report to NSF, "The Scientific Instrumentation Needs of Research Universities," provides a striking example of the increased costs young researchers face as they seek to establish their research programs (see table on following page). The data now are somewhat dated, so the message they convey should be interpreted as a conservative estimate of present costs. In the case of a leading chemistry department, the costs associated with equipping a new faculty member to do up-to-date research in the field of synthetic organic chemistry increased from $116,500 in 1970 to $741,000 in 1979. Inflation accounts for only part of the rising costs. Imposed on the rising baseline of inflation are much larger costs resulting from advances in existing instrumentation and the development of totally new, more powerful, and, therefore, more productive instruments. Young researchers of promise, who are yet without successful track records, cannot meet these costs without substantial assistance from their departments and institutions; and they are increasingly hampered in being able to respond.

The National Science Foundation has proposed a new Presidential Young Investigators Awards program that will provide no more than 200 recent Ph.D.s with 5-year awards averaging $30,000, to be matched from private sources. The funds will enable these potential new researchers to undertake a research program promptly and to have a genuine opportunity to develop their skills as investigators in their early years. For some it will mean the difference between remaining in the academic environment and leaving to pursue more encouraging and fruitful alternatives.

Just as the Department of Energy has followed the lead of the National Science Foundation and the Department of Defense by establishing a new university research equipment program, so it should give serious consideration to undertaking a comparable program targeted on enhancing the human resource pool in energy-related fields. We suggest that the Committee consider adding such an initiative to the FY 1984 DOE authorization request.

**TABLE 4. Increased Start-up Costs for Frontier Research** (Synthetic Organic Chemistry)

| 1970 | | 1979 | |
|---|---|---|---|
| **Instrument** | **Cost** | **Instrument** | **Cost** |
| | **Laboratory Instrumentation** | | |
| Rinco evaporator - 2 | $ 300 | Rotavac evaporators | $ 1,700 |
| 2 vacuum pumps | 400 | Prep. liquid chromatograph with fraction collector | 7,200 |
| Distillation apparatus—spinning band | 825 | Analytical liquid chromatograph with recorder and data system shared with capillary column | 13,250 |
| Solvent stills - 3 | 600 | Gas chromatograph | 11,300 |
| Melting point apparatus | 150 | Solvent stills with fire safety hoods | 4,500 |
| Gas chromatograph and recorder | 1,450 | Hot plates, stirrers, and heating mantles | 600 |
| Hot plates and stirrers, heating mantles | 275 | Microware with standard taper joints | 3,000 |
| Glassware | 500 | Laboratory glassware | 800 |
| Infracord Infrared Spectrometer | 3,500 | Thin layer chromatographic plates and tanks | 1,500 |
| **TOTALS:** | $ 8,000 | | $ 43,850 |
| | **Departmental Equipment to Which Access Required** | | |
| A-60 NMR Spectrometer | $40,000 | 90 mhz R-32 NMR spectrometer | $ 52,000 |
| Cary 14 UV-Visible Spectrometer | 14,000 | 200 mhz wide bore NMR spectrometer | 225,000 |
| Hitachi RMU-6 Mass Spectrometer | 60,000 | CFT-20 $^{13}C$ NMR spectrometer | 80,000 |
| Precision refractometer | 2,500 | GC-mass spectrometer and data system - Finnigan 4000 | 160,000 |
| | | Cary 17 - UV, Vis, Near IR spectrometer | 32,000 |
| | | Digilab FT-IR spectrometer | 82,000 |
| | | High-resolution mass spectrometer - CEC - 21 - 110 | 110,000 |
| **TOTALS:** | $116,500 | | $741,000 |

## RESEARCH INSTRUMENTATION

The third area requiring attention is the need to address the serious problems posed for modern research and quality graduate education by obsolete research tools. The instrumentation and equipment now being used in many of our research and training laboratories are seriously out of date. As previously reported to the Committee by Dr. Robert Marston, President of the University of Florida, a study completed in 1980 for the National Science Foundation found that the median age of instruments then in use in 15 universities surveyed was twice that of the commercial laboratories studied. That report, and others, have been provided to the Committee in earlier hearings; the Executive Summary is attached to my statement for the record.

In three years, much has happened. Investment in research instrumentation by some federal agencies has increased significantly. The FY 1984 budget request for the National Science Foundation proposes a significant increase in support for equipment within its research programs. Among the mission agencies, the Department of Defense has recognized the consequences to its basic research effort of leaving the problem unaddressed. DOD this year is proposing the second year of a 5-year, $150 million program to upgrade research instrumentation in laboratories that carry out research programs of interest to the Department. An Interagency Task Force on University Research Instrumentation is continuing to explore the problem. Within the university community the AAU, joined by the National Association of State Universities and Land-Grant Colleges and the Council on Governmental Relations of the National Association of College and University Business Officers, hopes soon to undertake a special joint project to explore alternative approaches toward meeting the substantial backlog of obsolete equipment.

We are pleased to note that the Department of Energy also has joined in this effort. The FY 1984 budget request includes $6 million for a new program to begin to address the equipment needs of laboratories in which energy-related research is done. The new University Research Instrumentation (URI) program will provide for the purchase of modern instrumentation used in areas such as combustion, biological energy conversion, catalysis, radioactive waste management, and engineering research. This is an important first step toward addressing the equipment needs of these laboratories. We urge the Committee to fully support this important initiative as a starting point toward a sustained reinvestment strategy for the decade of the 1980s. We share the view of the Administration that the problem of obsolete research equipment is one that affects adversely all agencies that support basic science; all agencies share a responsibility to work toward solutions. The University Research Instrumentation program is an appropriate and necessary response by DOE. We believe the program should grow quickly to a level roughly equal to that of the new DOD program ($30 million per year) and be sustained for at least 5 years at that level. We ask the Committee to consider that level as an appropriate and necessary objective for this important new initiative.

## RESEARCH FACILITIES

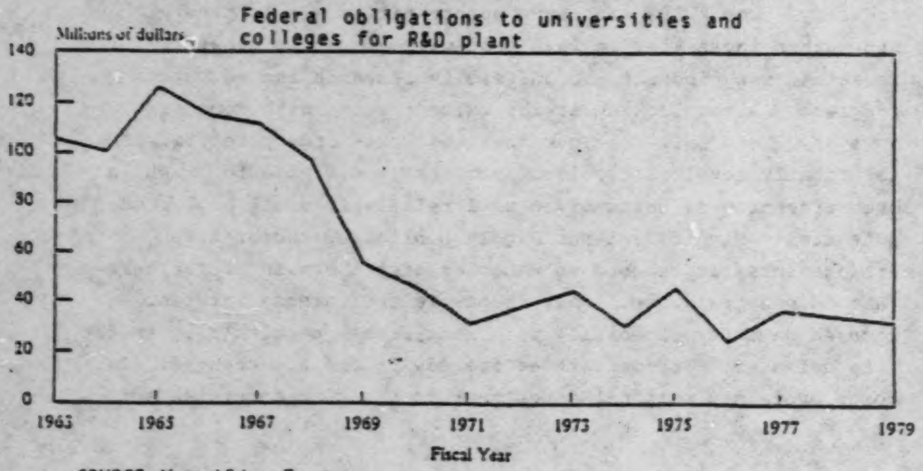
Turning to my final point, the need also to address the problems associated with deteriorated research laboratories, I find that the Department, in its budget request, states the general problem well, at least as it affects certain DOE facilities. In requesting $40 million for the Multiprogram General Purpose Facilities (MGPE) the Department in its budget justification states the following:

> "The program is based on the premise that through continuous usage and aging, facilities deteriorate over time to a point where they are no longer useful to perform their intended functions and

must be rehabilitated or replaced."
Source: FY 1984 budget request, p. 121,195.

In facing the serious problems of the DOE multiprogram laboratories, the Department is addressing the same problems spawned by obsolete facilities faced by many university researchers who seek to carry out DOE research programs in outdated laboratory buildings. The need to replace, renovate, rehabilitate, and reequip old research laboratories is the other half of the research equipment problem. In many ways the equipment and facilities problems are intertwined. But, as a 1981 survey of research facility needs, earlier provided to the Committee, points out, our leading research universities have substantial backlogs of research facilities in need of replacement, or at least modernization, if they are to be suitable sites for the conduct of state-of-the-art DOE-funded research programs. (The Executive Summary from that report also is attached to this statement.)

As shown by the following figure, for more than 15 years the federal government has invested essentially no funds to assist institutions in the replacement and repair of those laboratories in which the nation's basic science programs are conducted.


Federal obligations to universities and colleges for R&D plant

SOURCE: National Science Foundation

Many of these buildings were constructed 25 or more years ago in response to a national mandate; now many of those facilities need the same attention required by the DOE General Purpose Facilities--but for many financially pressed institutions the resources to do this job simply are not available. Since the alternative is the progressive deterioration of research laboratories essential to the Department's mission, and further deterioration in the quality of the science and education programs carried out in them, we believe it is time for the leading agencies of the federal government that fund the nation's basic science programs to assist in the rehabilitation of those laboratories essential to each agency's research mission and goals.

In the case of the Department of Energy, we ask the Committee to consider establishing a program to rehabilitate DOE-funded university laboratories at a level equal to the Multiprogram General Purpose Facilities program--$40 million.

## CONCLUSION

We are hearing much these days, Mr. Chairman, about the potential to meet the nation's needs for research and highly trained people by forging closer ties between universities and industry. There are important opportunities, it is true, to strengthen those ties in ways that will result in greater investments by industry in university research and education programs. After looking at the current scene with some care, however, I can only conclude that the long-range potential here is probably considerably less that many would hope for. While much attention is being given to a relatively small number of potentially significant and highly publicized cooperative arrangements, it is good to remember that there is little here that is genuinely new. What is new is the intense interest focused on a few especially promising areas, particularly in the life sciences. For many other fields, little has changed. For most, sustained national investment in basic science, funded through the agencies of our federal government, must continue to be the primary, even sole, source of research support.

The FY 1984 budget request by the Department of Energy sustains and strengthens the Department's commitment to university research programs. We ask the Committee to fully support the Department's request and to build upon those proposals along the lines suggested here.

I appreciate the opportunity to appear before the Committee today. I will be pleased to answer any questions you may have.

# THE SCIENTIFIC INSTRUMENTATION NEEDS OF RESEARCH UNIVERSITIES

A Report to the National Science Foundation

by the

Association of American Universities

June 1980

# The Scientific Instrumentation Needs of Research Universities

## EXECUTIVE SUMMARY

### OBJECTIVES OF THE STUDY

- assess the current status of scientific instrumentation in major research universities
- identify factors which facilitate or impede its development, acquisition, use, and maintenance
- estimate the future consequences of present funding policies
- recommend appropriate policy changes

### METHODOLOGY

- sixteen universities, six national and government laboratories, and nine commercial laboratories were visited; about 80 department heads and over 250 researchers and research administrators were interviewed in the universities; in addition, the views of approximately 60 researchers and research managers were gathered in visits to national, government, and commercial laboratories
- five fields of science and engineering were studied: physics, chemistry, biological sciences, earth sciences, and electrical engineering
- numerical data on instrumentation acquisition, age, and replacement were gathered from 14 universities and four commercial laboratories

### THE PROBLEM

Instrumentation is essential for progress in experimental research. Scientific knowledge has advanced rapidly; new instruments, more costly but qualitatively superior to earlier ones, have allowed measurements previously impossible. The rate of change has been great, competition among researchers intense. The capacity of university researchers to work at the frontiers of knowledge has important consequences for the nation's future research productivity and for the training of future generations of scientists and engineers.

The federal government has adopted a variety of funding policies for the support of research instrumentation: the competitive project system, special instrumentation programs, formula grant programs, block-funded centers, federally sponsored research centers, and regional instrumentation centers chief among them. For about 15 years, federal support for basic research has gradually declined; now support levels are further strained by rapid inflation. But inflationary price increases are simply the rising baseline upon which are superimposed the frequently much larger increases resulting from instrumentation advances.

### ASSESSMENT

The great diversity of instrumentation needs found among institutions visited precludes simple generalizations. However, problems in the acquisition, use, and maintenance of research instrumentation were found in all of the leading universities visited. The precise configuration of these needs and problems varies widely. The capabilities of institutions and departments to meet them do also.

University researchers face great difficulty in replacing worn-out or obsolete instrumentation and in acquiring new instruments needed for frontier research. A comparison of university instrumentation inventories with those of two leading commercial laboratories reveals that the median age of university equipment is *twice* that of the commercial laboratories' instrumentation. Even the nation's major research universities have not kept pace in meeting instrumentation needs. Researchers heavily dependent upon federal funds have witnessed grant size fall behind escalating research requirements. As a result, many work with inadequate tools that impair the pace of research, force closure of lines of inquiry, strain operations and maintenance budgets, and compromise the advanced education of graduate students.

Start-up funds for new faculty often are shrinking as costs rise. Support equipment is needed to test, calibrate, and augment primary instruments. Often it is especially difficult to fund. Fu[illegible] operation and maintenance costs is increasingl[illegible]lt to obtain. As a result, faculty and students spen[illegible]search time to these routine tasks.

Many research facilities are in need of renovation or replacement; federal funding for these purposes has decreased, however, and universities lack the funds to respond. Signs that aging facilities now impede research were found in many laboratories and support facilities.

Local university needs vary widely, reflecting the unique circumstances under which each institution operates and the great diversity between and even within the five fields studied. Present funding policies do not provide sufficient flexibility to deal with such local circumstances.

When compared with government and commercial laboratories visited, most university laboratories fare poorly. The best-equipped industrial laboratories surpass almost all university laboratories visited. One university researcher captured the sense of disparity and observed, "the ivory towers are now in industry." The director of one leading industrial research laboratory described the struggle of university researchers as he has observed them as "pathetic." When asked to contrast their laboratories with foreign laboratories, a number of university researchers stated that many, most notably those in Japan and West Germany, are now superior.

## CONCLUSION

The quality of research instrumentation in major university laboratories has seriously eroded. Not all, but many researchers in the nation's best-funded universities are struggling to work effectively with obsolete tools. The equipment being used in the top-ranked universities has a median age twice that of the instrumentation available to leading industrial research laboratories.

Three *causes* are identified:

- a relative decrease in instrumentation support
- inflexibility within the project system
- insufficient support for maintenance

Three major *consequences* are:

- diminished research productivity
- reduced training capacity
- decline in international competitive status

## RECOMMENDATIONS

Federal policy for the support of research instrumentation should provide for a basic three-part funding strategy:

- strengthen instrumentation funding in the project system
- expand special instrumentation programs
- create in the National Science Foundation a new, supplemental formula grant program to provide needed flexibility to meet diverse institutional needs

## FUTURE STUDIES

Five studies are recommended: 1) a thorough analysis of foreign research funding policies and programs and their implications for the competitive stature of American science should be undertaken; 2) a study of cooperative research arrangements between industries and universities and the factors—including federal policies—that encourage and discourage them is needed; 3) the planned assessment of regional instrumentation centers should include consideration of the concerns documented in this report; 4) universities should assess current instrumentation policies and develop mechanisms and incentives designed to promote effective sharing of instrumentation; 5) an assessment of the costs and benefits of federally-mandated equipment inventory and reporting systems should be undertaken.

# THE NATION'S DETERIORATING UNIVERSITY RESEARCH FACILITIES

## A Survey of Recent Expenditures and Projected Needs in Fifteen Universities

July 1981

Association of American Universities
One Dupont Circle, N.W.
Washington, D.C. 20036

# EXECUTIVE SUMMARY

## OBJECTIVES OF THE SURVEY

This survey of 15 leading universities had three objectives:

-- In the institutions surveyed, document expenditures for research facilities and major ($100,000 or more) equipment, without regard to source of funds, over the last four years in the following six fields:

- Biological Sciences
- Chemical Sciences
- Earth Sciences
- Engineering
- Physics
- Medical Schools (10)

-- Provide responsible estimates, prepared by those universities, of expenditures for research facilities and major equipment that would be required over the next 3 years to meet the research and training needs of only current faculty.

-- From researchers and department heads, acquire assessments of the major consequences for science, industry, and government if the accumulating facilities needs of their institutions are not addressed.

## FINDINGS

In many of the science and engineering departments surveyed, a substantial backlog of research facilities and equipment needs is accumulating. Projected funding needs over the next three years equal almost twice the level of expenditures by universities for their research laboratories and equipment over the past four years. Already, institutions often are unable to provide their basic science and engineering researchers and students with either the modern laboratories or equipment needed to conduct state-of-the-art research and education programs.

The loss of federal contributions to the support of research facilities and the absence of compensatory sources of support has forced many institutions to renovate and repair only those facilities in greatest need. Even this work often is carried out in a necessarily piecemeal fashion which is uneconomical over the long term. Working under such conditions, researchers unavoidably compromise their productivity and future competitiveness.

Relatively few construction projects have been completed in recent years in a majority of the physical sciences and engineering departments surveyed. Many institutions estimate substantial needs for new laboratories, for the expanded modernization and repair of old laboratories and for modern research equipment. Details are provided in the following report and the appendicies. The survey's principal findings are the following:

- In the last four years, for the six fields surveyed, fifteen leading universities together spent $400 million from all sources for facilities and major equipment. Half of those expenditures was for a relatively small number of new construction projects; the remaining half, spread over a larger number of generally less costly funding projects, was for facility modernization, major repair and renovation, and special purpose research equipment.

- In the next three years, the 15 universities estimate they should spend almost twice as much ($765 million) just to provide the necessary research facilities and special research equipment for current faculty only. These estimates are in current dollars and, therefore, do not include the added effects of inflation.

- New construction to replace outmoded facilities accounts for almost 60% of total projected funding requirements across all fields.

- In addition, substantial needs for major research equipment were identified in all six fields. These equipment estimates include only major items costing at least $100,000. As the 1980 AAU report to the National Science Foundation found, most departments also have serious deficiencies of less costly analytical instrumentation and support equipment.

## RECOMMENDATIONS

The problems of obsolescence in the nation's university research facilities have been growing for more than a decade. To solve them, a new national investment strategy is needed. That strategy should provide direct, balanced support for university facilities from federal agencies and provide investment incentives designed to involve industry, the states, and the universities in a coordinated effort. The following steps are suggested as starting points:

1. Key Executive agencies should develop a mechanism to periodically assess the condition of university research laboratories and the consequences for the nation of that condition. Such assessments should involve the Office of

Science and Technology Policy, the National Science Foundation, the Department of Defense, the Department of Energy, the Department of Health and Human Services, the Department of Agriculture and the National Aeronautics and Space Administration. The views of industry and the universities also should be solicited.

If the initial review corroborates the assessment of this suvey the following recommendations should be considered:

2. The facilities and equipment program earlier proposed to be undertaken by the National Science Foundation but deferred should be restored as a priority initiative in FY 1983.

3. The Department of Defense, Department of Energy, the National Aeronautics and Space Administration, the Department of Health and Human Services, and the Department of Agriculture should establish research instrumentation and facilities rehabilitation programs targeted on the fields of science and engineering of primary significance to their missions. The National Science Foundation should assist in the development, implementation, and coordination of those activities.

4. Incentives should be provided to encourage industries and the states to join with universities and the federal government in renewing the nation's university research laboratories.

5. Interest costs incurred in the construction, modernization, renovation, and repair of facilities in which federally supported research programs are carried out should be allowable indirect costs under OMB Circular A-21.

Mr. **Fuqua**. Thank you very much, Dr. Rosensweig.

Dr. Brinkman, there are several new or expanded materials facilities that are proposed in the DOE fiscal year 1984 budget, the one at the Lawrence Berkeley lab, at Brookhaven, and at Oak Ridge. Do you think that these have had adequate review by the scientific community?

Dr. **Brinkman**. Well, let me comment on them individually.

The Brookhaven laboratory is what they call phase II. It is effectively building additional laboratory space around the light source, the National Synchrotron Light Source. That situation out there, if you go and look at it, is very overcrowded, the whole facility, and there is hardly any question about the need for that additional building. It is a relatively small part of the total package, I think, that you are confronted with.

The High Temperature Materials Laboratory at Oak Ridge has been discussed for a number of years. I do not know if that is in your purview. I believe it is in conservation. It has certainly had wide exposure to the community.

Now, the Berkeley initiative, it seems to me, involves a number of things, and you have to be careful about it. There is a small part of it which involves the SSRL at Stanford. The idea of doing what they want to do there has been discussed extensively in the community over the years.

The National Center for Advanced Materials proposal at Berkeley is something that, as far as I know, people heard about—people like me, I should say—heard about it last summer. The community has had some of its first. exposure in the last couple of months.

Now, where are we with respect to reviewing that facility? A committee was formed in January to review the technical feasibility of the light source, and that review seems to have successfully passed. However, there has not been a great deal of scrutiny as to whether it is the next light source we would like to have as a Nation. Is it the right direction?

This particular light source is designed and directed toward the soft X-ray or ultraviolet light experimental programs, and that community has not had a chance to examine what this source would do for them.

I should say that my own opinion is that the University of California is one of our very best universities in material science and produces many students every year. Currently, LBL, supports something like 350 students doing graduate study and research. So it is a place where we get a lot of scientists. The concept of having this National Center for Advanced Materials at Berkeley, at a National Laboratory that needs redirection, is a good one.

I do think, however, that some scrutiny of the proposed program is exactly what is needed at this stage.

Mr. **Fuqua**. Dr. Schreisheim, my question concerns the chemistry programs that we have, the Federal programs. Do you feel as though there is adequate coordination between the programs that are Federally funded, particularly in the field of chemistry?

Dr. **Schriesheim**. That is indeed a difficult question to answer. By coordination between the Federal programs in chemistry——

Mr. **Fuqua**. Well, we have NSF, DOE, some of the different agencies all funding work in chemistry. My concern is, is there a lot of unnecessary work, is there duplication, or is there some overall coordination by the Federal Government for this kind of work?

Dr. **Schriesheim**. From the standpoint of an industrial research director who has responsibility for allocating resources in a resource-limited environment, from that standpoint, I would have to answer no, my perception is that there is not that kind of coordination and priority setting.

Now, then, of course, it becomes how much is enough and what are the roles and functions of each individual agency, and so forth. And I need to qualify my statement by pointing out that neither I nor the board on chemical sciences has studied this coordinating issue.

Mr. **Fuqua**. Mr. Mineta?

Mr. **Mineta**. Thank you very much, Mr. Chairman.

I would like to ask Dr. Brinkman about the National Center for Advanced Materials concept or lab that is being suggested in the new budget from the President.

Evidently, from your own lab, Dr. Anderson has said that this really has not been subjected to enough peer review, as I gather from his letter of February 14 to Chairman Fuqua.

You are probably aware that Dr. Trivelpiece is setting up an ad hoc committee. I don't know the membership of that committee, but do you think that that kind of committee is the thing that the materials science community is looking to for the, quote, adequate peer review, unquote, of what is being suggested for Berkeley?

Dr. **Brinkman**. Yes. I would like to make an added remark if I may about the status of our review. The committee that Dr. Trivelpiece discussed as being set up is a step in the right direction.

The way these national facilities generally evolve is that there is a lot that has to occur. For example, usually you will call several workshops. You would not only get a prestigious national committee, but you would get people who would want to use the facilities to come in and look at it and see what it is going to be, and you would change it as a function of time.

That evolution can occur. There is no reason why that should not occur and I think it will occur. But those are the kinds of things that have to happen here now to give a real concrete form to that proposal. It takes a lot of time and evolution to get the kind of thing that you want when you try to build something like this, and it is very, very important that we understand that.

One of the points that Mr. Keyworth made in discussing this was that we have limited resources and we cannot do everything. So when you cannot do everything, you have to be sure that you are trying to do the thing that is going to lead to the best possible science. You try to make that decision.

When it comes to national facilities, it seems to me that that decision has to be made with an enormous amount of interaction, because the give and take is often the most important part in knowing about what is going on. So I think these interactions all have to happen as a function of time, to make sure we are making the right decisions.

What I said earlier is that I like the basic concept, but I think the program has to be gone through very carefully.

Mr. **Mineta**. Should it be broken up into its four component projects and each sent off to a different lab?

Dr. **Brinkman**. I don't know that that particularly helps or hinders. I think that you ought to think about this proposal as a Berkeley proposal at the moment and try to see that it is properly formulated for a Berkeley proposal.

The national laboratories ought to make their own proposals as to what they want to do.

Mr. **Mineta**. At this point, though, it is more than just a Berkeley proposal, is it not? It is an administration-endorsed approach.

Dr. **Brinkman**. It certainly is more than a Berkeley proposal in the sense that it is a national facility, but it is a national facility at Berkeley.

Mr. **Mineta**. But it has gone through the Office of Science and Technology Policy.

Dr. **Brinkman**. That is right.

Mr. **Mineta**. It has gone through the Department of Energy. I assume that, in going through those steps, they have looked at the proposal as well as what facilities are available and how we should deal with this whole area of materials science.

Dr. **Brinkman**. Well, I know the people in the Department of Energy have thought a lot about this thing in the last several months, and so has, obviously, Mr. Keyworth's office. There is no question about that.

What I said earlier that had not happened is that the community had not had an opportunity to look at it.

Mr. **Mineta**. Would you acknowledge that there is a need for some kind of long-term, basic materials research program?

Dr. **Brinkman**. Absolutely. My opinion is that one of the things we face in this country—I particularly think about Bell Laboratories, and I am sure it is true of Exxon and other places—is finding well-trained people in the field.

As I said earlier, one of the attractive features of this proposal is that it is at LBL, which is closely coupled with the University of California at Berkeley, which would be a very good resource not only for doing basic research but for students being trained in this field.

Mr. **Mineta**. Mr. Chairman, if I might just impose upon you for one quick question of Dr. Rosenzweig on his testimony relative to instrumentation.

You called very clearly for the increased program in human resources, just as we have done in research instrumentation. But I am even wondering, in research instrumentation, whether or not more of that money has not been in computers rather than in scientific research instruments, that is, in instrumentation in that sense rather than computers. I sometimes wonder whether or not the words do not get used interchangeably, or whether that is in my own mind.

Dr. **Rosenzweig**. I am not referring primarily to computers but to the broad run of scientific instrumentation, which in most university laboratories now is 10 to 15 years behind comparable instruments in industrial laboratories.

Mr. **Mineta**. Thank you very much, Bob. It is good seeing you here.

Dr. **Rosenzweig**. Thank you.

Mr. **Mineta**. Thank you, Mr. Chairman.

Mr. **Fuqua**. Thank you, Mr. Mineta.

Mr. Sensenbrenner?

Mr. **Sensenbrenner**. Thank you very much, Mr. Chairman.

I apologize for being late today, but I was detained at another hearing on a piece of legislation that I cosponsored. I have an opening statement which has already been submitted for the record.

I have one quick question that I would like to ask of each of the three panelists. As you know, the budget proposal that we are considering treats the basic sciences quite generously, particularly in the context of other domestic spending programs. For example, the basic energy sciences budget is increased by 14 percent above the fiscal year 1983 appropriation. The nuclear physics budget is increased by 16 percent, and the high energy physics budget is increased by 17 percent.

Given the pulling and tugging that is sure to go on here when the final budget is put together, those kinds of increases make a pretty good target for those who would like to cut, shift, and divert elsewhere. So I think we are going to have a fight maintaining those figures.

I am concerned that we might not be able to absorb this increase within the fiscal year if we are able to succeed in getting the increases that I just stated. Do you think that we would be able to do that?

Dr. **Rosenzweig**. Do you mean to use the money effectively?

Mr. **Sensenbrenner**. Yes, sir.

Dr. **Rosenzweig**. Oh, yes. I think we are starting from a low enough base and with a large enough amount of slack that has been built up in the system for the last 7 or 8 years that the money can be used effectively.

Mr. **Sensenbrenner**. Can you tell us exactly how you think that money can be used effectively and absorbed during the period of fiscal year 1984?

Dr. **Rosenzweig**. Well, I cannot tell you in detail because I am not in command of the science personally. It may be hubris on the part of the scientists, but I have never heard any of them express a doubt that they could spend the money wisely if it were available to them.

Mr. **Sensenbrenner**. The problem is that, as we heard back when the synthetic fuels bill was passed here 3 years ago and subsequently, that was not proven to be the case.

Dr. **Rosenzweig**. I think the question, Mr. Sensenbrenner, or the answer to the question depends on how precisely targeted the money is for research. The more focused the money is, as a general proposition, the lower the capacity to use it. The more broadly the money is available for the lines of inquiry that the basic scientists themselves judge to be important, the larger the capacity is to use it, because there is less redirection of their efforts that is required.

If money is used not to buy a product but to support a broad line of research, I think the capacity unquestionably exists. If it is used to buy a specific product, as it was in some of the earlier programs, then I think the field could be quite rapidly satiated.

Mr. **Sensenbrenner**. I think, Dr. Rosenzweig, your argument is very persuasive in this room because we have some familiarity with how the money that is authorized under the jurisdiction of this committee is spent. However, that argument is not going to be persuasive once we leave this room and a priority-type budget is put together, because, as you said, the less strings attached, the less specific the authorization is, the better it can be utilized. I don't think that will sell across the street.

Dr. **Rosenzweig**. We count on your eloquence, Mr. Sensenbrenner.

Mr. **Sensenbrenner**. Thank you.

Dr. **Brinkman**. I would just like to make a couple of comments. One of them is, if you look at the proposal of DOE here, there is no question about how the money will be spent. They are well-defined programs.

In addition, it seems to me, you could look at some other very simple evidence that it would be rather easy to spend the increase and spend it well. For example, the Department of Defense, I think, recently ran a program for instrumentation. There was some number of millions of dollars they were willing to spend on new instrumentation at universities. I am sorry I do not recall the exact amount.

They got a number of proposals for that for some enormous amount more than the money they had to spend. I think it was 10 times or more than the amount of money they had to spend on that program.

So it seems to me that it is quite clear that there is a backlog of funding that can easily absorb any increase that you give here. In NSF, the number of proposals being turned down is quite high, and many of them are quality proposals. So I really have no question. I don't know what kind of ammunition to give you to go to your colleagues on other committees, but that seems to be quite apparent.

Dr. **Schriesheim**. Certainly with respect to the area that I know the best, the chemical sciences area, we have identified, as I pointed out in my testimony, a cornucopia of challenges, basic research challenges, underlying scientific sticky points that, in my view, and certainly in the view of the National Research Council committee, are not really attacked in the current climate by industry or by universities or by many of the national labs, not for a lack of ideas or for a lack of the specific areas in which to work, but really for a lack of funding.

With respect to the basic energy research component, it is the kind of funding that you put into a program at the very beginning in order to either develop ideas or further develop concepts.

I don't think we would really have a problem identifying energy chemistry oriented ideas and concepts that could easily soak up these kinds of funds. The NRC Board on Chemical Sciences and Technology would be glad to work with you on doing that.

Mr. **Sensenbrenner**. Thank you very much, gentlemen.

Mr. **Fuqua**. Dr. Rosenzweig, many people in the university community have related to us that the new changes that the National Science Foundation has made in their grants, whereby they provide more latitude in the management of the research programs, have worked very well and will consequently result in more research than we were getting in some of the other management methodologies.

Do you think that DOE should consider a similar approach to funding research? I am speaking particularly of universities.

Dr. **Rosenzweig**. I think the short answer to that is yes, and I am not enough of a specialist to give you the long answer to it. But the experience with the NSF approach has been a good one, on the whole, and I think there is reason to believe that it is extendable in principle to other agencies and with some profit.

Mr. **Fuqua**. The DOE research instrumentation initiatives differ from those of NSF and DOD. DOE's support for new instruments would not be tied to specific research proposals as is somewhat done in NSF and DOD.

Do you think that the DOD approach is better? What are the advantages or disadvantages for some of these?

Dr. **Rosenzweig**. I think a sensible program would combine additional funds for instrumentation that are associated with specific pieces of research and a more general program to reequip laboratories so that they become functioning modern laboratories out of which research projects will emerge in the future.

I do not think that either approach exclusively addresses the problem by itself, but the two combined, it seems to me, make the sensible package, and that is what we would like to see.

Mr. **Fuqua**. Let me ask all three of you, if you care to comment about it—if you want to plead the fifth amendment, OK—do the DOE laboratories have an unfair advantage over universities in competing for DOE research funds?

Dr. **Rosenzweig**. That is a tough question, and I don't think there is a single answer to it, because the process is complicated. My sense of it is that the laboratories have an advantage in the formulation of the budget. They get to it first and have their voice heard first. But universities may have an advantage in the legislative process, in the making of appropriations, because we are more free to speak our needs where those needs exceed whatever the administration has proposed.

I am not sure where the balance is struck between those two. I guess, if I had my choice, I would rather be in their position than in our position, but that is about as far as I think I can go on it.

Dr. **Brinkman**. Let me comment on the question.

It seems to me, as I look at materials science, that the situation is the following. The university program in the DOE has increased over the last years quite enormously. The programs at the national laboratories have increased slightly but primarily only on the basis of national facilities that have been proposed to be run by these laboratories.

In fact, the core research programs at the national laboratories have suffered and decreased quite enormously over the years. So if I just look at the evidence, I would say, at least on proposals for regular research programs, the universities are doing better than the national laboratories. So the national laboratories do not have an undue advantage at this stage. At least that is the impression I get.

Dr. **Schriesheim**. We have examined that issue slightly, Mr. Chairman, as part of that study that I mentioned. We have generally concluded that the labs do not have an advantage that would be a significant advantage, in your terms.

Of course, I would like to simply say that it is really quality research that we are interested in, and that should be the underlying criterion, anyway, whether it is a national lab or a university research program.

Mr. **Fuqua**. Mr. Mineta?

Mr. **Mineta**. Thank you, Mr. Chairman.

Let me ask Dr. Rosenzweig, as you are very well aware, all the Department of Energy basic research support is through contracts rather than grants. I would like to ask whether or not the Department of Energy should consider supporting university researchers through grants rather than contracts.

Dr. **Rosenzweig**. Yes; I would like to see that happen. One can work out an acceptable arrangement under a contract, certainly, and I think they have been worked out, but from the university's point of view, a grant is a better instrument because it is more flexible and better suits the nature of what happens in university research. That is to say, a contract is something you use generally to buy a product, and while that can be tailored to what university laboratories do, it is a bit of a stretch sometimes, and it would be, I think, more straightforward and in better policy if—

Mr. **Mineta**. Just out of curiosity, from an administrative oversight responsibility, is there a greater burden on the university through a grant as compared to a contract?

Dr. **Rosenzweig**. No; I don't believe so. I don't think that is the case.

Most of the universities—in fact, all of the universities—that are heavily engaged in Government sponsored research have become pretty accomplished at managing the programs and have a mix of grant and contract instruments in support of their research. I think the differential management burden is probably minimal by now.

Mr. **Mineta**. Dr. Schriesheim, I am wondering, is there a, quote, big science, small science, unquote, conflict within the chemistry research community?

Dr. **Schriesheim**. Well, I don't think so. The question of big and little science, at least in my mind, is much more a quality issue. It is not a size issue. It is what really should be supported within the constraints of limited resources, and that has to be that research which is of the highest quality, regardless of size.

Mr. **Mineta**. Do you think the DOE chemical science program is meeting those needs?

Dr. **Schriesheim**. By and large. I think that the chemical research program at DOE is like many other programs of this size. It must be viewed as a portfolio. The DOE chemical sciences portfolio certainly is, in my view, a quality portfolio, whether it is conducted at the national labs or in a university. It could always be improved upon, but it is basically high quality.

I might say, sir, that I cannot resist making a comment on the question you asked Dr. Rosenzweig, and that had to do with grants versus contracts. In the study that we had conducted of the DOE basic energy sciences program, another recommendation of our committee was that grants or contracts or what have you at the universities should, to the extent possible, be multiyear rather than single year, because multiyear funding obviously provides a certain stability to the research professor and his students.

Mr. **Mineta**. Dr. Brinkman, often there is an adversarial relationship between Government and the private sector. I am just wondering, how can the Department of Energy foster a greater collaborative effort between the national laboratories, or the laboratories of the Department of Energy, and industry?

Dr. **Brinkman**. Well, I tried to say something about that in my testimony in the sense that I think, if you look at the national facilities that we talk about in the materials science, and the neutron-scattering facilities, the reactors and the light sources, the synchrotron sources, those things are very much based on a collaborative effort between industry and the national laboratories.

Dr. Schriesheim has just cited how much money Exxon has put into this. I could cite the amount of money that Bell Laboratories has put into these facilities. We have had people working out at Stanford from year 1 when that facility first turned on, and we have had people working out at Brookhaven doing neutron scattering since the mid-1960's and earlier.

So these unique facilities have played a very, very important role in coupling the national laboratories to industries.

Now, the other one I tried to cite in my testimony was, for instance, the program in Oak Ridge where they have a research program in what I call ion-solid interactions and laser annealing. It is a specific program in materials research. They have done research in that, but they set up this equipment, and a variety of companies have gone there and tried things out. They don't have that kind of equipment themselves, so they have been able to go and try these things.

With respect to basic research, it is these kinds of things that the national laboratories can contribute to industry in major way.

Berkeley is another place. They have the National Center for Electron Microscopy now. That is a thing that costs a lot of money. They have an atomic resolution microscope—and it is a high-energy machine. I am sure that the industrial laboratories will go there to use it.

There are places like Arizona, for instance—that is a university project—where people do go and use electron microscopes there.

And so it is these kinds of specialized things, not always huge facilities like the neutron scattering and the synchrotron facilities, but unique facilities that we find attractive.

We cannot do everything in a given industrial laboratory. Industries, because of antitrust laws, of course, cannot cooperate on these kinds of things very much. So these laboratories can act as focal points for various types of research.

I think there could be more of that on things of smaller scale than the facilities.

Mr. **Mineta**. Thank you very, very much.

Thank you, Mr. Chairman.

Mr. **Fuqua**. Thank you, Mr. Mineta.

Let me express, on behalf of the subcommittee, my appreciation to all three of you for being here. I think it has been very helpful, and I think you have contributed a great deal to a better understanding of something that I think we all mutually are interested in.

Thank you very much for being here.

The subcommittee will be adjourned.

[Whereupon, at 10:47 a.m., the subcommittee recessed, to reconvene at the call of the Chair.]

# 1984 DEPARTMENT OF ENERGY AUTHORIZATION

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## THURSDAY, MARCH 3, 1983

House of Representatives,
Committee on Science and Technology,
Subcommittee on Energy Development and Applications,
*Washington, D.C.*

The subcommittee met, pursuant to recess, at 9:34 a.m., in room 2318, Rayburn House Office Building, Hon. Don Fuqua (chairman of the subcommittee) presiding.

Mr. Fuqua. The subcommittee will be in order.

I would like to ask our distinguished witnesses to join each other at the table.

This is our third hearing of the subcommittee on the fiscal year 1984 budget request for the Department of Energy's basic research programs.

The administration's request provides for real growth in basic energy sciences, in high energy physics, in nuclear physics, and it also includes initiatives for new research facilities at the national laboratories.

It is these new facilities that are the subject of today's hearing. The Department of Energy, which is among the top three Federal agencies in basic research funding, has assumed a special role in providing stewardship for the large, complex facilities for frontier research, such as the powerful particle accelerators, electron microscopes, radiation sources, and special testing laboratories.

These facilities are most often sited at the national laboratories because that is where the technical and management expertise is available to build as well as operate them. The facilities are often made available to qualified users from the scientific research community.

We are pleased to see that new and expanded facilities are being proposed in the budget. However, we want to be assured that every opportunity is being provided for collaboration among the laboratories, universities, and industry, as appropriate to each facility.

During this hearing, we want to find out more about the new facilities being proposed, how they will serve the research community, and how they will advance U.S. science and technology.

We also would like to ask our witnesses how the Federal Government and the Department of Energy should set their priorities for future facilities, particularly in times of constrained budgets.

Today's witnesses represent those DOE national laboratories that have major facility initiatives in the fiscal year 1984 budget. They are also distinguished scientists who have made significant contributions in their individual fields of research.

Our panel witnesses will be Dr. Nicholas Samios, Director of the Brookhaven National Laboratory; Dr. David Shirley, Director of the Lawrence Berkeley Laboratory; Dr. Alexander Zucker, the Associate Director of the Oak Ridge National Laboratory; and Dr. James D. Bjorken, senior scientist at the Fermi National Accelerator Laboratory in Illinois; and Dr. Burton Richter, Technical Director of the Stanford Linear Accelerator Center.

We will begin with you, Dr. Samios.

If you have prepared statements and you wish to make them part of the record and summarize, we will be happy to accommodate that. Once we hear from all of you, then we have some questions that I think will be interesting to all of you. Maybe someone would like to comment on the answers that another gave or contradict or add to or take away from the previous answers.

We may have some questions like that.

Dr. Samios?

## STATEMENT OF DR. NICHOLAS P. SAMIOS, DIRECTOR, BROOKHAVEN NATIONAL LABORATORY

Dr. **Samios**. I submitted a written testimony, but I will be briefer in my verbal remarks.

Mr. **Fuqua**. And we will make it, in its entirety, part of the record.

Dr. **Samios**. Fine.

I am pleased to appear before you today to testify on the proposed construction of phase II of the National Synchrotron Light Source, the NSLS.

The NSLS, located at Brookhaven National Laboratory represents a very large step forward in the utilization of ultraviolet and X-ray radiation for a broad range of disciplines, including advanced very small scale electronic device fabrication, or X-ray lithography, surface and condensed matter science, biophysics and chemistry.

NSLS consists of two storage rings individually optimized to provide high brilliance in the ultraviolet [UV] and X-ray spectral regions, respectively. The importance of this facility is evidenced both by its machine performance parameters and by the degree of commitment of the scientific and technological communities.

The unexpectedly large initial response of these communities, as well as new scientific opportunities, underlie the planned phase II upgrading at this time.

Although this construction project ended only in September 1981, there are already representatives of 27 universities, 12 industrial laboratories, and 6 national laboratories grouped into 27 participating research teams.

There are two classes of users at the NSLS. The 27 participating research teams, PRT's, are consortia of industry, university, government, and national laboratory teams with major research programs centered at the NSLS. They have committed resources and personnel for the construction and operation of beam lines on which they will utilize up to 75 percent of the operating time. The remaining time is available to general users.

There are now some 185 PRT members, with the following representation: Industry, 45; universities, 78; national facilities, 62. These participating research teams have committed a total of $18 million toward beam lines.

The second category of users is those who have less substantial synchrotron radiation research programs and who will apply for usage of existing NSLS and PRT lines. To date, we have had over 1,000 individuals inquiring about the general user program, which is scheduled to begin in the spring of 1983: Industry, 231; universities, 621; national facilities, 178. These numbers point to a heavy oversubscription for available beams.

There are two aspects to the new science envisaged for phase II. The first will utilize existing but uninstrumented ports and will work at the present high-spectral brilliance. Most of these additional ports are presently inaccessible because of building space limitations.

To accommodate many approved experiments and requests, we propose to expand both the X-ray and the UV experimental floors and add a mezzanine to the UV building to accommodate this experimental demand as well as to provide additional laboratory and office space for the many users.

The second aspect involves adding and upgrading beam lines. Wiggler devices, which require straight sections in the storage rings, will produce flux enhancements of factors of 10 to 100 over conventional bending magnet sources for a broad spectral range. Likewise, undulator sources can provide even greater brightness enhancements at specific tunable frequencies, with the further advantage that they can greatly diminish unwanted radiation. We plan to add nine new beam lines, two-thirds of which will involve such wigglers and undulators.

This project was first proposed to DOE for inclusion in the fiscal year 1982 budget, when it was realized that the demand for facilities at the NSLS was greatly accelerated compared to our expectations. The proposal has benefited from considerable interaction with the user community in defining the parameters of the expansion. Our Users' Executive Committee has had input to the proposal, and it was discussed twice at our annual users' meetings, at which there were 300 registrants.

I must say that this interest in synchrotron radiation and the use of facilities such as NSLS, which has attracted a much larger number of scientists and at a much more rapid time scale than most optimistic estimates, is indeed gratifying. We are pleased to be able to respond to this groundswell of demand by university, industry, and laboratory users, and I look forward to completion of phase II in 1986.

I would now like to show a few transparencies and then finish up my testimony.

Mrs. **Bouquard** [as acting chairman]. Please proceed.

Dr. **Samios**. The first one is in the handout. It is a list of the university users, industry users, and national lab users of the synchrotron light source. It is a rather large list.

The second involves the distribution of beam line funding in the X-ray and UV rings, among university, industry, and Government users. You can see the amount of investment by each of the three sectors—approximate $6 million each—and how it is distributed between X-ray and UV rings.

Since the NSLS exists, we thought it would be useful to show what it looks like. The ensuing transparencies show the present building, a rather modern yet efficient structure, the floor layouts for both the UV and X-ray rings and the planned expansion areas. The last one consists of a photograph of the floor area at the UV facility taken 2 weeks ago indicating the already crowded conditions and intense utilization of this facility.

Before concluding my testimony, I have been asked, and it is appropriate, to make a few brief remarks concerning the CBA facility which has been under construction at BNL since 1979.

You will recall that construction was virtually curtailed pending resolution of the magnet design issue. At that point, this project was approximately one-third complete. We have taken advantage of this pause to reexamine physics, management, technical, and cost issues.

I would like to report on several recent major developments. First, I am pleased to announce that Paul Reardon accepted the position of associate director for high-energy facilities and director of the CBA project on October 1, 1982. Mr. Reardon comes to BNL from Princeton, where he was project manager of the Tokamak Fusion Test Reactor which was recently successfully completed. This was the latest of a long string of his successes in the management of large projects in high energy physics and in fusion.

The R&D program has been highly successful in that magnets that are presently being produced meet or exceed design specifications for the CBA. In addition, we have recently installed and successfully tested at full magnetic field a full cell of eight magnets in the existing tunnel. The emphasis is now in establishing a magnet production capability assuring accelerator quality magnets at reasonable cost.

Dr. Trivelpiece testified on February 24, 1983, at the House Committee on Science and Technology that DOE plans an indepth review of this project in April, a Woods Hole and HEPAP review in June, and a DOE decision in July on the construction of a colliding beam accelerator at BNL.

With our major technical problems behind us and the reaffirmation of the exciting physics possibilities enunciated in the Snowmass Review by the Division of Particles and Fields of the American Physical Society last summer, we look forward to a strong endorsement for CBA and the resumption of construction.

In closing, I should note that New York State, through the Power Authority of New York State, PASNY, has provided cheaper power, up to 30 megawatts, to BNL for the past 2 years. This has greatly aided us in maintaining and expanding our important programs in research and in the operation of these power-dependent facilities.

I would like to close by showing three transparencies.

Mrs. BOUQUARD. Please.

Mr. SAMIOS. Where previously I showed a picture of the light source, I will now show an aerial view of the laboratory as a whole. One notes the large circle in the north area, the CBA tunnel, and experimental areas which are 70 percent complete.

To illustrate the progress in magnetic of development, I show a curve of a magnetic field coefficient plotted as a function of current in the magnet. The important feature is that all the points fall within the shaded region.

Last is a transparency of the magnets, the eight magnets comprising a full cell in the CBA tunnel. These have successfully been cooled and energized to the required magnetic field—several times. A major step.

Thank you.

[The prepared statement of Dr. Samios follows:]

## TESTIMONY OF DR. NICHOLAS P. SAMIOS

I am pleased to appear before you today to testify on the proposed construction of Phase II of the National Synchrotron Light Source (NSLS). The NSLS, located at Brookhaven National Laboratory, represents a very large step forward in the utilization of ultraviolet and x-ray radiation for a broad range of disciplines, including advanced very small scale electronic device fabrication (x-ray lithography), surface and condensed matter science, biophysics, and chemistry. The importance of this facility is evidenced both by its machine performance parameters and by the degree of commitment of the scientific and technological communities from universities, industry, and national and government laboratories. The unexpectedly large initial response of these communities, as well as new scientific opportunities underlie the planned Phase II upgrading at this time. The NSLS is a user facility with substantial involvement of universities, industry, and national laboratories. Although its construction project ended only in September 1981, there are already representatives of 27 universities, 12 industrial laboratories, and six national laboratories grouped into 27 participating research teams (PRTs).

NSLS consists of two storage rings individually optimized to provide very high brilliance in the UV and x-ray spectral regions, respectively. The x-ray ring is the only one in the U.S. specifically designed for synchrotron radiation, and the only one operated solely for the production of photons. Over much of the spectral range of major scientific and technological interest, NSLS is designed for significantly greater brilliance than any other source now existing or under construction.

Table 1 summarizes the experimental status worldwide. By the end of 1983 NSLS will have 45% of the U.S. total, and 19% of the world total of experimental lines. Note, however, that because of the high flux and brilliance at NSLS, this facility represents a substantially higher fraction of experimental utility.

There are two classes of users at NSLS. The 27 Participating Research Teams (PRT's) are consortia of industry, university, and government and national laboratory teams with major research programs centered at NSLS. They have committed resources and personnel for the construction and operation of beam lines on which they will utilize up to 75% of the operating time. (The remaining time is available to general users.) There are some 185 PRT members at present, with the following representation:

Industry - 45

Universities - 78

National Facilities - 62

These participating research teams have committed a total of $18 M towards beam lines. Breakdown is shown in Tables II and III.

The second category of users is those who have a less substantial synchrotron research program, and who will apply for usage of existing NSLS and PRT lines. To date we have had over 1000 individuals inquiring about the general user program, which is scheduled to begin in Spring, 1983 (Industry - 231; Universities - 621; National Facilities - 178). These numbers point to a heavy oversubscription for available beams. Note that 70% of these have never previously utilized synchrotron radiation.

There are two aspects to the new science envisioned for Phase II. The first will utilize existing but uninstrumented ports, and will work at the present high spectral brilliance. Most of these additional ports are presently inaccessible because of building space limitations. There are now two already approved x-ray groups (microprobe, atomic physics) and two approved UV groups for which appropriate space must be found (Exxon, Cornell, University of Tennessee consortium). At a recent meeting of the Program Advisory Committee, several new proposals were approved. These include the use of Compton backscattering to produce highly polarized and collimated x-ray photons with energies up to 600 MeV for nuclear physics research. Also approved is a University of California, Los Alamos, Lawrence Berkeley, Sandia Laboratory consortium proposal for a broad program including surface science, x-ray scattering, and standardized sources for calibration of detectors relevant to the weapons program. In addition, there was approval for a request for Stanford University. This was for an experimental line for non-invasive angiography research on human patients, the first such facility in the world. To accommodate these approvals and requests we propose to expand both the x-ray and the UV experimental floors and add a mezzanine in the UV building to accommodate this experimental demand as well to provide additional laboratory and office space for the many new users.

The second aspect involves adding and upgrading beam lines. Wiggler devices, which require straight sections in the storage rings, will produce flux enhancements of factors of 10 to 100 over conventional bending magnet sources for a broad spectral range. Likewise, undulator sources can provide even greater brightness enhancements at specific tunable frequencies, with the further advantage that they greatly diminish unwanted radiation. There are 8 straight sections on the x-ray ring and 2 on the UV ring. One of the x-ray straight sections is reserved for Compton backscattering, leaving 7 for insertion devices. One of the UV straight sections is already occupied by an undulator device for a free electron laser experiment. These devices will have the same increase in scientific capabililty as has been experienced in the use of conventional synchrotron sources compared with laboratory x-ray tubes. We plan to add 9 new beam lines; two thirds of which will involve such wigglers and undulators. We anticipate that this will open up new opportunities in:

1. X-ray microscopy and holography.
2. X-ray microprobe.
3. Crystallography of single surface, monolayers, and small clusters.
4. High energy resolution inelastic x-ray spectroscopy.
5. Photochemistry and photochemical kinetics.

This project was first proposed to DOE for inclusion in the FY 1982 budget, when it was realized that the demand for facilities at the NSLS was greatly accelerated compared to our expectations. The proposal has benefitted from considerable interaction with the user community in defining the parameters of the expansion. Our Users' Executive Committee has had imput to the proposal, and it was discussed twice at our annual Users' Meetings, at which there were 300 registrants.

I must say that this interest in synchrotron radiation and the use of facilities such as NSLS, which has attracted a much larger number of scientists and at a much more rapid time scale than most optimistic estimates, is indeed gratifying. We are pleased to be able to respond to this groundswell of demand by university, industry, and laboratory users, and I look forward to completion of Phase II in 1986.

Before concluding my testimony, I have been asked and it is appropriate to make a few brief remarks concerning the CBA facility which has been under construction at BNL since 1979.

You will recall that construction was virtually curtailed pending resolution of the magnet design issue. At that point this project was approximately 1/3 complete. We have taken advantage of this pause to reexamine physics, management, technical and cost issues. I would like to report on several recent major developments. First I am pleased to announce that Paul Reardon accepted the position of Associate Director for High Energy Facilities and Director of the CBA Project on October 1, 1982. Mr. Reardon comes to BNL from Princeton where he was Project Manager of the Tokamak Fusion Test Reactor which was recently successfully completed. This is the latest of a long string of his successes in the management of large projects in high energy physics and fusion.

The R&D program has been highly successful in that the magnets that are presently being produced meet or exceed design specifications for the CBA. In addition, we have recently installed and successfully tested at full magnetic field a full cell of eight (full scale, full length) magnets in the existing tunnel. The emphasis is now in establishing a magnet production capability assuring accelerator quality magnets at reasonable cost.

Dr. Trivelpiece testified on February 24, 1983 that DOE plans an in depth review of this project in July, a Woods Hole and a HEPAP review in June, and a DOE decision in July on the construction of a colliding beam accelerator at BNL.

With our major technical problems behind us and the reaffirmation of the exciting physics possibilities enunciated in the Snow Mass Review by the Division of Particles and Fields last summer, we look forward to a strong endorsement for CBA and the resumption of construction.

TABLE I

| Country | City | Ring | Stge Ring | E GeV | E KeV | # Ports Now | # Exp. Stas. Now | Pable. Future Ports | Pable. Future Stas. |
|---|---|---|---|---|---|---|---|---|---|
| France | Orsay | ACO | * | 0.55 | 0.33 | 4 | 12 | | |
| | | DCI | * | 1.8 | 3.39 | 2 | 10 | 2 | 10 |
| Germany | Braunchweig | PTB | | 0.14 | 0.013 | 2 | 2 | | |
| | Bonn | BONN I | | 0.5 | 0.163 | 1 | 1 | | |
| | Bonn | BONN II | | 2.5 | 4.53 | 4 | 4 | | |
| | Hamburg | DESY | | 7.5 | 25.5 | 3 | 9 | | |
| | Hamburg | DORIS | * | 5.0 | 22.9 | 10 | 29 | | |
| | Hamburg | PETRA | * | 19.0 | 75.0 | – | – | 1 | 1 |
| | Berlin | BESSIE | * | 0.8 | 0.63 | 11 | 11 | 13 | 61 |
| Italy | Frascati | ADONE | * | 1.5 | 1.5 | 2 | 6 | 2 | 4 |
| Japan | Tokyo | INS-SOR I | | 1.3 | 1.22 | 1 | 1 | | |
| | Tokyo | INS-SOR II | | 0.3 | 0.054 | 4 | 6 | 2 | 2 |
| | Tsukuba | Photon Fac | * | 2.5 | 4.3 | 6 | 27 | 14 | 56 |
| | Tsukuba | | * | 0.6 | 0.3 | 4 | 4 | 12 | 36 |
| Sweden | Lund | LUSY | | 1.2 | 1.05 | 1 | 2 | | |
| U.K. | Daresbury | SRS | * | 2.0 | 3.2 | 2 | 8 | 10 | 40 |
| USSR | Moscow | C60 | | 0.68 | 0.35 | | | | |
| | Moscow | PACHRA | | 1.3 | 1.22 | | | | |
| | Yerevan | ARUS | | 6.0 | 19.5 | | | | |
| | Tomsk | SIRIUS | | 1.3 | 11.6 | | | | |
| | Novosibirsk | VEPP-2M | * | 0.67 | 0.35 | | | | |
| | Novosibirsk | VEPP-3 | * | 2.2 | 3.8 | | | | |
| | Novosibirsk | VEPP-4 | * | 6 | 14.5 | | | | |
| USA | Stoughton | ALADDIN | * | 1.0 | 1.06 | 20 | 20 | 16 | 16 |
| | Stoughton | TANTALUS I | * | 0.24 | 0.048 | 7 | 9 | | |
| | Washington | SURF II | * | 0.24 | 0.037 | 6 | 9 | 1 | 2 |
| | Stanford | SPEAR | * | 4.0 | 11.2 | 5 | 14 | 8 | 10 |
| | Stanford | PEP | * | 15.0 | 44 | – | – | 1 | 1 |
| | Cornell | CESR | * | 8.0 | 35.0 | 3 | 3 | – | – |
| | **BNL NSLS** | UV | * | 0.7 | 0.40 | 9 | 19 | 7 | 22 |
| | **BNL NSLS** | X-RAY | * | 2.5 | 4.25 | 18 | 34 | 10 | 20 |

## NSLS PARTICIPATING

## RESEARCH TEAM MEMBERS

### UNIVERSITIES

Brandeis University (X12)
Columbia University (U9)
Georgia Institute of Technology (X14)
Harvard University (X12, X13)
Johns Hopkins University (X19)
Massachusetts Institute of Technology
  (X14, X20)
New York University (X9)
North Carolina State University (X11)
Northwestern University (X18)
Princeton University (X18)
Purdue University (X18)
Rensselaer Polytechnic Institute (X19)
State University of New York
  (U7, U15, X21)
University of Connecticut (X11)
University of Houston (X14)
University of Illinois (X18, X19)
University of Kentucky (X14)
University of North Carolina (U11)
University of Pennsylvania
  (U12, X9, X12, X13)
University of Pittsburgh (X13, X18)
University of Rochester (U11)
University of Washington (X11)
Virginia Polytechnic Institute (X14)
West Virginia University (X18)
Yale University (U11, X12)
Yeshiva University (X9)

### CORPORATIONS

Allied Corporation (X13)
Ashland Oil (X18)
Bell Laboratories (U4, X9, X15, X16)
Dupont (X11)
Exxon (X10)
General Electric (X11)
Gulf (X18)
IBM (U6, U8, X20)
Mobil (X11)
United Technology (U11, X11)
Universal Oil Products (X18)
Xerox (U12)

### NATIONAL LABORATORIES

Ames Laboratory (X18)
Argonne National Laboratory (U11, X11)
Brookhaven National Laboratory
  (U7, U9, U11, X11, X12, X13, X22)
National Bureau of Standards (X23, X24)
Naval Research Laboratory (X23, X24)
Oak Ridge National Laboratory
  (U11, U12, X14)

CAPITAL INVESTMENT IN BEAM LINES
($K)

| | University | Industry | Government |
|---|---|---|---|
| X-ray | 5300 | 3190 | 3950 |
| UV | 760 | 4100 | 1850 |
| Total | 6060 | 7290 | 5800 |

University: 32%; Industry: 38%; Government: 30%.

Mrs. **BOUQUARD**. Thank you very much, Dr. Samios.

The next witness is Dr. David Shirley, who is director of the Lawrence Berkeley Laboratory. You may proceed as you wish, Dr. Shirley.

## STATEMENT OF DR. DAVID SHIRLEY, DIRECTOR, LAWRENCE BERKELEY LABORATORY

Dr. **SHIRLEY**. Madam Chairman and honorable members of the committee, it is a pleasure to testify today about the National Center for Advanced Materials at the Lawrence Berkeley Laboratory, which is included in the Department of Energy budget request for fiscal year 1984.

I have submitted written testimony, and I will not go through it in detail, but I would like to extract some highlights of it, with some personal comments.

Mrs. **BOUQUARD**. Dr. Shirley, it will be made a part of the complete record.

Dr. **SHIRLEY**. Thank you.

I believe that 1983 is going to be remembered as the year that America reawakened to the importance of science education and high technology to securing its future in energy, its physical security, and its economic security. I would like to see initiatives take place in this year and in coming years somewhat analogous to the response that America made to the Sputnik in 1957. I am proud to be part of these initiatives.

Within this context, the DOE multiprogram national laboratories are emerging from a period of uncertainty about their role in the future. It was a hearing before this committee in July 1981 on the role and mission of national laboratories which led to a number of reviews. Again on June 2 of last year, there was a hearing before this committee which addressed budget uncertainties and their impact on national laboratories. I participated in that hearing, along with a number of other laboratory directors.

In response to the perception by the national laboratories that they should reexamine their priorities and redirect their programs to address national needs, I am going to describe today a proposal by Lawrence Berkeley Laboratory to address a national need in advanced materials research.

This is a response to a number of challenges—challenges from the White House Science Council Panel on Federal Laboratories, which was set up, I believe, as a result of the July 1981 hearing, by the Department of Energy's Energy Research Advisory Board, and by other review bodies, and culminating last December in hearings before this committee.

Our laboratory has reexamined its strengths in close collaboration with the Office of Energy Research in the Department of Energy and as part of our institutional planning process for the laboratory to generate the proposal that is before you now.

We propose to provide major new facilities to help maintain U.S. technological leadership. We would do this through generating new knowledge in advanced materials, through training of scientists and engineers in collaboration with the university sectors, and through close coupling of basic research to the needs of America's high-technology industry.

The principal scientific goal will be an enhanced understanding of atomic interactions that underlie the synthesis, the characterization, and the properties of advanced materials. The center, NCAM, which we propose, would consist of two specially equipped laboratory buildings that would enable researchers to pursue long-range basic research in surface science and catalysis, advanced materials synthesis, and advanced device concepts.

The centerpiece of NCAM would be a light source of advanced design which we call the advanced light source. By providing brilliant beams of synchrotron radiation in extremely fast pulses, the advanced light source would open the way to new classes of studies in materials science.

A recent report by the National Academy of Sciences, which in fact is not yet formally available, attests to the importance of synchrotron radiation in scientific research, particularly the accelerating demand which is projected by this report to exceed supply by the mid-1980's. I would just underline Dr. Samios' comments that the NSLS is already oversubscribed.

The facilities that we are proposing would be coordinated with the Stanford Synchrotron Radiation Laboratory, which provides higher energy X-rays by synchrotron radiation, and it would complement SSRL when it is complete. It would also, of course, be coordinated with NSLS.

We are determined to make NCAM a truly national facility. I would like to emphasize that we are particularly determined to involve American industry in the very fabric of NCAM. In fact, before I return to California, I shall be visiting four major industrial laboratories on the east coast—Bell Laboratories, General Electric, IBM, and Exxon—to discuss ways in which they can participate in formulating and conducting the scientific program at NCAM.

I would like to describe briefly several formal mechanisms that we would like to establish to insure that NCAM is a truly national facility.

First of all, Dr. Trivelpiece testified last week on NCAM, and he indicated that he was prepared to set up an ad hoc committee made up of advisers from industry and universities to give him input on the scientific program of NCAM.

We propose also to set up a scientific advisory board which would have strong input from scientific leaders from industries, universities, and Federal laboratories and would advise us on formation of the detailed scientific program in the outyears.

We are proposing an NCAM industrial fellows program which would foster joint research projects with industry and facilitate the participation of industrial scientists in NCAM basic research studies of mutual interest to the laboratory and to the scientists' parent companies.

We propose NCAM affiliates, which will be analogous to the PRT concept now in operation at NSLS and SSRL.

And, finally, we would have a program advisory committee analogous to program advisory committees that already exist to allocate beam time at our other accelerators, in this case for the advanced light source.

In formulating these plans, we have sought advice and comment from industry leaders throughout the country, and we have received many letters of support for the concept of NCAM. In fact, I have been surprised that the support has been so strong and essentially unanimous from industrial scientists.

I would like to stress two things about NCAM. First, the national laboratories have demonstrated the capability to build and operate large research facilities. A report was prepared at the request of the Energy Research Advisory Board of DOE which describes the wide range of user facilities in operation at various national laboratories. Included at Lawrence Berkeley Laboratory is the National Center for Electron Microscopy, which is just now coming up to speed. It is extremely important to materials research. It includes the capability to study important materials at the atomic level. Also, the Bevalac, our preeminent heavy ion accelerator, has been managed as a national facility for some years by the laboratory.

I would like to say a little bit about the evolution of NCAM. First of all, there is a strong base for it at our laboratory. LBL has been, for many years, one of the world leaders in accelerator physics. The laboratory is over 50 years old, and a number of the prototype accelerators for nuclear physics were originally built at LBL.

More recently, we have had close collaboration with SLAC in the design and construction of an electron storage ring, PEP.

In the field of synchrotron radiation, we have had strong participation at SSRL in synchrotron radiation research since 1974. As a matter of fact, LBL is the second largest user of SSRL after the Stanford complex itself.

We have taken a leadership position in the design of wigglers and undulators, which are the insertion devices used for very intense synchrotron radiation sources. We in particular have pioneered the design of permanent magnet wigglers and undulators, and we presently are engaged in a joint collaborative effort with Exxon research and with SSRL to build a 54-pole wiggler which is presently being completed in our shops and will soon be installed at SSRL.

So, both from the perspective of accelerator design generally and from the perspective of synchrotron radiation specifically, the laboratory has been in a very good position to create a program in synchrotron radiation.

Now, LBL is unique among the national laboratories in being located adjacent to, and having strong ties with, a leading research university, the University of California at Berkeley. There are presently—we just counted the other day—172 Berkeley faculty members engaged in research at Lawrence Berkeley Laboratory, and the thesis research of more than 500 resident graduate students is pursued with sponsorship of LBL.

This includes a wide range of science, physics, chemistry, materials science, and some participation from the College of Engineering. NCAM would greatly strengthen that participation.

The laboratory contributes substantially to highly trained manpower in the area of energy research. I might just comment that in each of the four laboratories that I will be visiting next week, former doctor of philosophy students of my own are presently working.

Materials research has been assigned very high national priority in a variety of contexts. Several years ago, I participated in the solid state sciences committee symposium at the National Academy in a review of the importance of advanced materials research. Unfortunately, the report is not available yet but it did address the importance of the subject.

When materials research was assigned high national priority, laboratory resources have been mobilized to determine the role that Berkeley can play. We expect synchrotron light sources to be the most important class of research tools for materials science during the next decade.

Last April, LBL established a team to design the first of the next generation light sources, which we now call the advanced light source. We presented our ideas to DOE last June and to visiting committees charged with reviewing national laboratories in July.

The formal DOE review process began in September as part of the institutional planning process of the Office of Energy Research. In November, members of the DOE's Office of Management examined our construction plans and cost schedules in detail.

In January, there were two reviews. There was a technical review of the advanced light source by technical leaders in the synchrotron radiation field from other facilities in the United States and from the French machine at Orsay. This review led to the report which I will now quote, in summary:

> That the advanced light source extends the design of existing machines to new limits, with new challenges, and will be a powerful research tool in the years ahead.

The next week, on January 10, representatives from the Department of Energy visited the laboratory to review our program plans for fiscal year 1984. Also in attendance were members of the Council on Chemical Research, which represents some 40 industries and the university sector, and members of the Solid State Sciences Committee of the National Academy of Sciences.

This process of review will continue. Workshops will be held this spring on the design of beam lines for the advanced light source and on specific program areas such as electronic materials. Plans for these workshops are already underway.

I am determined to make every effort possible to involve the entire user community in the process of shaping NCAM.

In conclusion, I would like to pledge my full cooperation as you go through the process of weighing this request on its merits and in the context of the Nation's overall science effort.

Finally, in closing, I would like to note an article that I saw in the Washington Post this morning to the effect that the House of Representatives passed a bill yesterday which is described as the first step toward addressing an existing national problem, the shortfall in science and engineering education. I would like to applaud that step. I think this is part of the same issue we are addressing.

Mrs. **Bouquard**. We thank you. We are very happy that we were able to pass this much-needed legislation.

Dr. **Shirley**. I think that this country is in a race in high technology, and I think the race has already started, and I am very pleased to see America getting out of the blocks.

Thank you. I am ready for questions.

[The prepared statement of Dr. Shirley follows:]

# TESTIMONY OF DAVID A. SHIRLEY, DIRECTOR OF LAWRENCE BERKELEY LABORATORY, BEFORE THE SUBCOMMITTEE ON ENERGY DEVELOPMENT AND APPLICATIONS

Mister chairman, honorable members of the committee, it is a pleasure to testify here today about the National Center for Advanced Materials (NCAM) at the Lawrence Berkeley Laboratory, which is included in the Department of Energy budget request for FY 1984.

Our great multi-program national laboratories are emerging from a traumatic period of uncertainty about their role and future. This committee held hearings on that subject in July of 1981 and again on June 2 of last year. In the latter hearing, I testified that our Laboratory was the hardest hit by sudden budget cuts and changes of program direction, which caused us to lose 19% of our work force. Now I bring a happier message - that we at Berkeley propose to implement American science policy by redirection of a national laboratory to address a vital national need in advanced materials research.

This initiative is proposed in the context of answering the challenge to the national laboratories -- heard repeatedly in reviews by the White House Science Council and the DOE Energy Research Advisory Board, and culminating last December in hearings before this committee. That challenge called on the national laboratories to consider their strengths, and undertake redirections, in order that their missions be appropriate for the 1980's, 1990's, and beyond. We have undertaken this reexamination in close collaboration with the Office of Energy Research -- the office of the Department of Energy that is most closely concerned with the institutional goals and planning of our Laboratory.

The Administration's budget request for the Department of Energy, which is before you now, contains a proposal for a materials research center at the Lawrence Berkeley Laboratory that will provide major new facilities needed to help foster a secure energy future, enhance national security, and maintain U.S. technological leadership. It will do this through new knowledge, the training of scientists and engineers, and close coupling of basic research studies to the needs of America's high-technology industries. The Center's principal scientific goal will be an enhanced understanding of the atomic interactions underlying the synthesis, characterization, and properties of advanced materials. Its facilities and its research results will be readily accessible to all qualified researchers.

The Center will consist of two specially equipped laboratory buildings that will enable researchers to pursue long-range basic research in the areas of surface science and catalysis, advanced materials synthesis, and advanced device concepts. A synchrotron light source of advanced design (the Advanced Light Source, or ALS) will be the centerpiece of the proposed complex. By providing brilliant beams of synchrotron light in extremely fast pulses, the ALS will open the way to new classes of materials studies.

A recent report by the National Academy of Sciences attests to the importance of synchrotron light in scientific research, the strong interest from industry, and the growing demand, which is expected to outstrip the supply of synchrotron light sources by the mid 1980's. In addition to the facilities described above, which will be built at the Laboratory, some 10 per cent of the requested funds will go into improving an existing synchrotron light source at the nearby Stanford Synchrotron Radiation Laboratory (SSRL). When NCAM is complete, the two laboratories will complement one another.

Let me stress that we are determined to make NCAM a truly national facility, and that we are particularly determined to involve American industry in the very fabric of NCAM. Industry and universities will participate in many ways in the design and use of ALS beam lines and ancillary NCAM laboratories to assure that NCAM basic research addresses America's high-technology needs:

- An NCAM Advisory Board, composed of scientific leaders from industry, universities, and federal laboratories, will advise the Laboratory Director on NCAM scientific program directions.

- An NCAM Industrial Fellows program will be established to provide joint LBL-industry support for participation of industrial scientists in NCAM basic research studies of mutual interest to LBL and the scientists' parent companies.

- The NCAM affiliates, consisting of major research teams from industry and universities, will participate in the design, construction, and use of ALS beam lines.

- An ALS Program Advisory Committee, comprised of synchrotron light experts from across the nation, will review research proposals for the use of ALS beam lines.

In developing these plans, we have sought advice and comment from industry leaders and have received many letters of support. Now that this project is before you for consideration, plans for more formal involvement of the nation's scientists are being put into action.

It is important to stress two things about NCAM. First, the national laboratories in general and the Lawrence Berkeley Laboratory in particular have demonstrated the capability to build and operate such facilities to the nation's benefit. A report that was prepared at the request of DOE's Energy Research Advisory Board describes the wide range of national user facilities already in operation at the Laboratory. One of these, the National Center for Electron Microscopy, is extremely important to materials research and will complement the Advanced Light Source. The second point is that although a facility may be located at a national laboratory, all the programs it supports need not be. For example, half the research performed at the Laboratory's Bevalac heavy-ion accelerator is conducted by scientists from outside the Laboratory.

Let me speak briefly about the evolution of NCAM. Since its inception, the Laboratory has been a world leader in accelerator physics. Recent successful efforts include close collaboration with Stanford in the design of an electron storage ring (PEP), and the development of advanced insertion devices -- so-called wigglers and undulators -- for generating synchrotron light. These are now in use at SSRL. In our shops at Berkeley is the next step in insertion devices, a joint effort with Exxon, which is slated for installation at SSRL later this year. Materials research, including synchrotron radiation research since 1974, constitutes an area in which the Laboratory has been poised to expand in response to a national need.

There is another factor supporting the formation of NCAM at LBL. The Lawrence Berkeley Laboratory is unique among the national laboratories in being located adjacent to and having strong ties with a leading research university, the University of California, Berkeley. The Laboratory's research programs already include the participation of 150 Berkeley faculty members and the thesis research of more than 500 resident graduate students. Thus, the Laboratory contributes substantially to the pool of highly trained manpower in the area of energy research. NCAM will further enhance this contribution to the benefit of America's universities and industries.

When materials research was assigned high national priority, Laboratory resources were mobilized to determine what role Berkeley might play. Research areas that are underexplored and that could be expected to yield novel materials or device concepts for eventual exploitation by high-technology industries were identified. Because we expect synchrotron light sources to be the most important class of research tools for materials research during the next decade, a team of accelerator physicists set out to design the first of the next-generation light sources. Our ideas were presented to DOE and to the visiting committees charged with reviewing the national laboratories.

The formal DOE review process began in September as part of the institutional planning process of the Office of Energy Research. Last November, members of DOE's Office of Management examined our construction plans and cost and schedule estimates in detail. In January there were two reviews. The first was a technical review of the Advanced Light Source design, called at my request, by the technical leaders from the U.S. synchrotron light sources and the French machine (LURE) at Orsay. The committee concluded, and I quote, "that the ALS extends the design of existing machines to new limits, with new challenges, and will be a powerful research tool in the years ahead." The areas for improvement that they suggested are being fully explored.

The following week, on January 10, representatives from the Department of Energy visited the Laboratory to review our program plans for FY 1984. Also in attendance were members of the Council on Chemical Research, representing many industries and the university sector, and the Solid State Sciences Commmittee of the National Academy of Sciences. These participants raised critical issues and offered their advice.

This process of review will continue, as I am determined to make every effort possible to involve the entire user community in the process of shaping the Center. I wholeheartedly endorse Dr. Trivelpiece's plan -- that was presented to you in testimony last week -- to form an advisory committee to review the scientific program as this important project gets underway.

In conclusion, let me offer my full cooperation as you go through the process of weighing this request on its merits and in the context of the nation's overall science effort. I would be pleased to answer questions now.

DAVID A. SHIRLEY, Ph.D., Sc.D., Director, Lawrence Berkeley Laboratory; Professor of Chemistry, University of California, Berkeley; member of the National Academy of Sciences and the American Chemical Society; Fellow of the American Academy of Arts and Sciences, the American Physical Society, and the American Association for the Advancement of Science. He received the California Section Award (ACS) in 1970 and the E. O. Lawrence Award in 1972. He served as Vice Chairman and Chairman of the Department of Chemistry and Associate Director and Head of the Materials and Molecular Research Division before becoming Director of LBL in 1980.

Mrs. **Bouquard**. Thank you very much, Dr. Shirley.

Being the representative of the Oak Ridge community is both a challenge and an honor, and I am sure that members of this committee will agree with me when they hear from our next witness, Dr. Alexander Zucker, whom I represent, and who is associate director of the Oak Ridge National Laboratory.

Dr. Zucker?

## STATEMENT OF DR. ALEXANDER ZUCKER, ASSOCIATE DIRECTOR, OAK RIDGE NATIONAL LABORATORY

Dr. **Zucker**. Thank you, Madam Chairman.

Members of the committee, it is a great honor to be here today. In the interest of time, I would request that my written testimony be submitted for the record, and I will paraphrase it.

Mrs. **Bouquard**. Without objection, it will be made a part of the record.

Dr. **Zucker**. Thank you.

First I would like to address the need for a high temperature materials laboratory. High temperature materials are intimately connected with all aspects of energy production. The major limitation to energy production usually lies in materials.

The first important aspect of that is the change in efficiency as a function of temperature. If you increase the temperature of a powerplant by 200 degrees, you gain about 5 percent in efficiency. If some ceramic engine for an automobile or a truck were to be developed, the mileage would go up between 30 and 50 percent.

The second aspect of the high temperature materials related to energy has to do with reliability. As you know, many high-temperature devices have rather short lives and exhibit real difficulties in maintenance and reliability.

The third aspect is that the very feasibility of some energy developments is only made possible if new materials were to be developed for the production of energy.

We conclude, therefore, that the high-temperature materials research is necessary. This has been noted for many years and by many committees. The question that arises is whether the scientific community should do this research in a dispersed fashion or in a single laboratory.

A dispersed research program would lead to good research and many good ideas would no doubt be pursued. The problems with a dispersed effort are three: first, the loss of communication between the various laboratories in an area where such communication is essential; second, a duplication of expensive equipment which is necessary for work at these very elevated temperatures. Finally, the very nature of the problems requires a multidiscplinary approach. For example, to attack a problem in corrosion at high temperatures, you have to apply various scientific disciplines such as thermodynamics, diffusion theory, high-temperature chemistry, fracture mechanics, and also have access to supporting services such as alloy preparation and microanalytical procedures.

Mrs. **Bouquard**. Excuse me, Dr. Zucker.

Dr. **Zucker**. Yes?

Mrs. **Bouquard**. It will be necessary to have a short recess because of the call of the Chair. Congressman Fuqua should be back in a couple of minutes and resume the hearing. I apologize for the delay.

Dr. **Zucker**. All right.

[Short recess taken.]

Mr. **Fuqua**. The subcommittee will resume.

I apologize for the interruption, but we are in session this morning and have very important votes, like approving the Journal. Please proceed.

Mr. **Zucker**. I will continue with my description of the high temperature materials laboratory, Mr. Chairman.

The concept is a centralized laboratory that has a multidisciplinary staff, that has unique equipment, that pays attention to applied and basic research, and that is supported by the extensive logistic and other support of the Oak Ridge National Laboratory.

Physically, the high temperature materials laboratory includes about 43,000 square feet of laboratory and office space.. It is supposed to house about 75 professionals. Included in the funding for the laboratory is $3.5 million explicitly set aside for high temperature experimental equipment.

We envision a split of 60-40 between basic research and applied research for the DOE technologies.

The idea of making this a national user facility is firmly embedded in the concept of the high temperature materials laboratory. I would like to quote a few facts about the Oak Ridge user facilities.

The facilities that we have designated as user facilities have served 460 individual scientists in fiscal year 1982. Of those, about 10 percent, or 44, have come from industry; about 350 from universities and the rest from other laboratories.

On page 15 of my testimony there is a list of universities that have collaborated with one section of the high temperature materials program, namely our section that produces pure materials.

We have paid special attention lately to our relations with industry. We formed an industrial users' advisory committee in 1980. On the advice of that committee, we convened an information meeting for industrial managers of appropriate industries in April of 1981. The membership of the committee is on page 14 of my testimony. On page 7 I list four interactions that have taken place on account of that April 1981 meeting with International Nickel, with Bendix, with Babcock & Wilcox, and with Cabot.

More recently, connections have been made with Carborundum, Norton, American Feldmuhle, and ARCO on matters dealing mainly with ceramics. While these have not blossomed into collaborative work yet, we have every indication that they will.

I would like Mr. Chairman, now to show some recent results that the high temperature laboratory has obtained. We feel that high temperature materials research is urgently needed so we have reoriented many of our research programs to this end, and I would like to show some results obtained over the last year.

A new alloy has been developed at Oak Ridge National Laboratory that consists of nickel and aluminum and, in this case, nickel, aluminum, and iron. (Please refer to the first figure of my testimony.)

When we talk about high-temperature machinery for energy, we are talking of a regime between 300 and 600 degrees. An extension of this regime up to 1,000 degrees would be extremely desirable.

You can see that the new alloy, nickel aluminide, is much stronger than 316 stainless steel. It is stronger, by a very significant factor at elevated temperatures, than the supper alloy, hastelloy. When we add iron, it becomes an exceptionally promising alloy for all sorts of energy and nonenergy application.

You notice this green square up here. That result has been discovered just 2 weeks ago. By adding hafnium to the nickel aluminide we find that at 850 degrees Celcius we have a strength of about 100,000 psi, a very unusual thing. New things are happening almost daily.

It is interesting also to look at high-temperature creep, which really tells you how useful the material is at elevated temperatures. The lower the creep, the better the material.

Stainless steel, in comparison with the nickel aluminides, turns out to be very inferior. Hastelloy, which, as I said, is one of the best things we have, is about three times poorer when one looks at the creep at 760 degrees, Centigrade. (See the second figure in my testimony.)

Turning now to ceramics, we have done some work in toughening ceramics, in this case alumina ceramics, although we have worked with other ceramics as well. Here I show the fracture toughness of various materials like glasses, oxides, carbides, and nitrides. (See third figure in my testimony.)

Here I show alumina toughened by introduction of zirconium oxide. You see that the toughness of the alumina has increased by about a factor of 2½. Just to orient you, most metals will have a fracture toughness in these same units of about 50 or 60; aluminum, which is a rather weak metal, at around 15 or 20. So we are approaching the fracture toughness of aluminum and other soft metals.

Let me just show one other result, again in ceramics, whereby we replace the tungsten carbide cobalt cutting tools with titanium diboride. This is one of the projects that is being worked on at the same time by an industrial outfit and by us.

Here are our results for what is called flank wear. In this case, the lower the flank wear the better the tool. You can see that the titanium diboride ceramic cutting tools fall in the lower part of the best commercial tungsten carbide tools.

Now I would like to make some closing remarks.

We feel that high temperature materials science is extremely promising. We feel that there are many new things that will be opened by research at high temperatures and that we have really only begun to scratch the surface.

Mr. Chairman, I brought some samples of this nickel aluminide, if you would like to look at these. There are two samples in here. The crumbling material that you see there is the nickel aluminide as it has been known for many years, and the addition of 500 parts per million of boron has transformed this rather useless, crumbly black stuff into the shiny material whose properties I showed you.

The fact that you can add 500 parts per million of boron and produce a very useful new material is a clear indication that we have a field of science here that is going to be just enormously promising for energy and other areas of technology.

Thank you very much.

[The prepared statement of Dr. Zucker follows:]

# HIGH TEMPERATURE MATERIALS LABORATORY

Alexander Zucker
Associate Director
Oak Ridge National Laboratory

My name is Alexander Zucker. I am Associate Director at the Oak Ridge National Laboratory with responsibility for the physical sciences. I appear here today to discuss the High Temperature Materials Laboratory.

Energy development is closely connected with materials. Nearly every technology - nuclear fusion, breeders, advanced coal-burning plants, coal liquefaction technologies - is limited by some deficiency in materials properties at high temperature. There are problems of mechanical strength at elevated temperatures, resistance to radiation damage, a limited life of an alloy or a ceramic, corrosion effects multiply at high temperatures, etc. Perhaps most important is the fact that the efficiency of heat engines increases with increasing temperature. It may be useful to digress at this point and illustrate this fact by an example using a fossil-fired steam turbine electric generator system.

> First, a bit of history. The Hartford Electric Light Company built the first central station steam-electric plant in the United States in 1902. Steam conditions were 350°F and 180 psig. The heat rate (i.e., the ratio of thermal input to net electric output) was approximately 35,000 Btu/kWh or a thermal efficiency of about 10%.[1] Continued advancements in materials science have allowed steam conditions to increase dramatically. Today's state-of-the-art is considered to be 1000°F and 3500 psig with an efficiency of 37% and heat rate of 9200 Btu/kWh. (Systems designs, etc., have also contributed to increased efficiency of the plants.)

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[1] More often than not, heat rate rather than thermal efficiency is reported for fossil combustion plants.

Programs are underway around the world - with greatest effort and emphasis in Japan - to reach a system with throttle steam conditions of 1200°F and 5000 psig. Materials for the steam turbine appear to be the limiting factor for this system. As a further step, systems operating with steam at 1400°F and 7000 psig are being considered but could certainly not begin operation until after the turn of the century. New materials and materials technology will be essential for such a system.

The table below summarizes the three preceding cases for modern and future 800 MWe units.

Efficiency of Fossil Power Plants

| Steam Conditions (°F/psig) | Plant Efficiency thermal(%)/ heat rate (Btu/kWh) | Fuel Cost Savings ($ million/year) |
|---|---|---|
| 1000/3500 | 37/9200 | Base |
| 1200/5000 | 42/8100 | 6.5 |
| 1400/7000 | 47/7300 | 13.0 |

Efficiency is not everything. Frequently the reliability and the very feasibility of a technological development depends on a new material. A case in point is the development of a ceramic diesel engine for automotive use. High temperature alloys now in common use present yet another problem: they are very demanding of materials which are in short supply, and many of which are not available from mineral resources in the U. S. Materials such as chromium, cobalt, platinum, and tungsten are essential for current high temperature alloys, and we must often obtain these materials from countries that are not always reliable suppliers. New alloys and new ceramics are being developed which will be much less demanding of such exotic materials. I conclude, therefore, that high temperature materials research is highly desirable, that it is in the national interest, and that it supports many aspects of energy production and use.

Two approaches are possible in establishing a High Temperature Materials Research Program. One option is to disperse research support among a relatively large number of institutions. Alternatively, the work can be carried out in one centralized location. Both approaches have advantages and disadvantages.

The wide dispersal of research contracts would unquestionably provide support for university graduate research programs, and the natural competitiveness among institutions would ensure that problems are considered from a variety of points of view. A great disadvantage of this system is the loss of close communication occasioned by the geographical separation of individual research projects. A considerable duplication in equipment requirements is also inevitable. But the most serious problem of all stems from the complex nature of most high temperature materials problems. For example, in advocating increased research on corrosion of alloys, the participants in the Materials Sciences Workshop on Phase Transformations, Stability, and Materials Interactions listed as areas needing study those phenomena associated with adsorption, the mechanism of scale formation, mass transport in scales, scale adhesion, and associated changes in substrate chemistry and microstructure. Thus, an in-depth study of alloy corrosion requires expertise in thermodynamics, diffusion phenomena, high temperature solid state chemistry, fracture mechanics, and other aspects of the mechanical behavior of materials. In addition, alloy preparation facilities are necessary. The equipment needed to characterize oxide scales and their substrates ranges from devices for measuring reaction kinetics to the microanalytical tools required to define microstructural changes resulting from oxidation. It is true that different components of this problem can be worked on separately, but the task of developing a comprehensive model of high temperature corrosion is clearly far more difficult when the individual research projects are carried out in relative isolation. In fact, we suggest that probably the principal reason why research on high temperature materials lags behind the obvious technological needs is that such research can not be carried out effectively in small, independent research groups.

Many of these difficulties disappear when the research is performed in a centralized location. The size of the professional staff would be sufficient to provide the varied expertise needed to address high temperature materials problems. Because of the scope of the effort, it becomes feasible to assign a significant fraction of the staff to the critically important tasks of materials synthesis, preparation, and characterization. Duplication of equipment needs is eliminated. Communication is made very much easier, and the fitting together of the results of various individual projects proceeds naturally. We conclude, therefore, that the optimum climate for high temperature materials research is to be found in a large, multidisciplinary laboratory (i.e., an HTML).

Precedent for this conclusion is provided by the experience at Pratt and Whitney where the development of superalloys for high temperature, gas-turbine engines required the efforts of a 60-person research team. Although the scope of that work was narrower than anticipated in the HTML, the key to success was the existence of a large, integrated, multidisciplinary, high temperature materials laboratory.

A second example of the effectiveness of the "central laboratory approach" to the solution of materials problems is given by the experience of the 40-person task force organized within the Metals and Ceramics Division at ORNL to attack the problem of void formation and swelling in type 316 stainless steel as a result of radiation damage. Fundamental studies (funded by OBES) of the influence of minor alloying elements on the mechanism of void formation and the careful coordination of this research with parallel applied work led, in a matter of months, to the development of a promising modification of type 316 stainless steel that is currently being fabricated into core components for testing in the Fast Flux Test Facility. This same low swelling alloy is also the "Path A" prime candidate alloy for the fusion program.

In structuring the proposed HTML, we have adopted the concept of a large, multidisciplinary laboratory. A staff of approximately 75 professionals with appropriate support will be organized into six functional groups (Environmental Interactions, Mechanical Behavior, High Temperature Chemistry, Physical Properties, Structural Characterization, and Materials Synthesis). Each group would contain about 12 professionals. Thus, each group would be large enough to establish a range of interrelated fundamental and applied programs that address broadly a particular problem area in high temperature materials science.

Our experience in developing the low-swelling alloys demonstrates the effectiveness of combining fundamental and applied research, and we plan that the functional groups would contain both. The presence of this applied work in a group would provide a natural channel for the flow of ideas and results from the fundamental to the applied work, and the applied project as a whole would be expected to benefit greatly from multidisciplinary input represented by the work of the six functional groups.

A number of strengths of such an HTML suggest themselves.

- The HTML with its large staff of professionals will provide the kind of expertise necessary to address the broad multifaceted, multidisciplinary questions typical of high temperature materials research. Research will concentrate on properties of matter (metals, ceramics, and composites) at high temperatures (500-3000°C), a temperature regime that is relatively unexplored.

- The HTML is designed as a national user facility to industry, universities, and other laboratories. It will house a unique collection of specialized equipment needed for high temperature materials research, and this equipment together with the expertise of the permanent staff will make the HTML attractive as a user facility.

- The HTML will be able to respond in a coordinated fashion to a problem in the use of materials at elevated temperatures requiring immediate attention and understanding.

The High Temperature Materials Laboratory itself consists of a 43,000 sq. ft. facility that includes 3.5 M$ worth of specialized equipment. The estimated cost is 20.6 M$ for construction starting in FY-84. Current projections indicate that about 60 percent of the work will be of a long-term basic research nature funded by the Office of Basic Energy Sciences; 40 percent will be devoted to applied research in metallurgy and ceramics funded by the technology programs of DOE.

We envision that the HTML will much expand our already fruitful relations with industry and with universities. As the repository of unique instrumentation and as a center of multidisciplinary expertise in science and engineering, the HTML will provide a national focus for high temperature materials research. Indeed, relations with industry and universities in the high temperature materials area are even now of great interest to us.

Here are some of the things we have done. We established an Industrial Users Advisory Committee consisting of leaders in various industries (see Appendix I for membership). In April 1981, at the advice of the Industrial Users Advisory Committee, we held a review of ORNL materials research highlights to which we invited a number of officials and managers of the country's foremost materials industries. The following activities, now in place, are directly traceable to that meeting two years ago. The interactions are in two categories, active interaction and strong interest.

Active Interaction. During this period, agreements were made with four industrial firms whereby ORNL and the companies conduct joint R & D.

1. INCO Research and Development Center, Inc., Sterling Forest, Suffern, New York: INCO and ORNL are jointly conducting research on the oxidation of oxide dispersion strengthened (ODS) nickel-base alloys with the objective of obtaining fundamental understanding of the observed superior oxidation properties of ODS alloys. INCO has prepared the alloys, ORNL has conducted preliminary characterization of the materials, INCO is now oxidizing the specimens, and ORNL will perform post-oxidation analysis.

2. Bendix Advanced Technology Center, Columbia, Maryland: ORNL has conducted research on long-range ordered (LRO) alloys for the past five years. Bendix learned of this work and expressed interest in evaluating LRO alloys as friction materials for advanced brake applications. ORNL has provided selected LRO materials and these are now being tested by Bendix. Results to date look very promising.

3. Babcock & Wilcox, Lynchburg Research Center, Lynchburg, Virginia: ORNL conducted research on titanium diboride ($TiB_2$) based ceramics and obtained data which suggest that

the materials may be attractive as metal cutting tools superior to commercial WC-Co tool material. B&W offered to evaluate ORNL $TiB_2$ ceramics in extensive metal cutting tests. ORNL prepared the materials; B&W is currently conducting the tests.

4. Cabot Corporation, Kokomo, Indiana: ORNL has contracted with Cabot Corporation for the preparation of large heats of LRO alloy to determine processing modifications necessary for scale-up for these alloys. Some of the heats have been prepared and delivered to ORNL for characterization and property studies.

For cases 1, 2, and 3, the industrial companies have agreed to provide ORNL with all of their results and that the results will be public information. For case 4, processed metal billets are deliverable to ORNL.

Strong Interest. During CYs 1982 and 1983, representatives of several industrial ceramic companies visited ORNL to learn of research results for ceramic materials and the result has been strongly expressed interest by several in determining how they and ORNL can work together. Selected examples are given, and indicate the principal official who came to Oak Ridge.

1. Carborundum Advanced Ceramics Division, Niagara Falls, New York; R. C. Phoenix, Vice President and General Manager: Carborundum is very interested in the sol-gel processing work on silicon carbide being conducted at ORNL and has indicated potential interest in transferring this technology to Carborundum.

2. Norton Company, Worcester, Massachusetts; Robert Russell, Vice President: Norton is highly interested in ORNL-conducted work on sol-gel processing of silicon carbide and zirconia ceramics and special materials characterization facilities available at ORNL.

3. American Feldmuhle Corporation, Hendersonville, North Carolina; James Floyd, President: American Feldmuhle is highly interested in ORNL results on sol-gel processing of $Al_2O_3$-$ZrO_2$ and $ZrO_2$ ceramics and is considering mechanisms whereby the two institutions could work together.

4. ARCO Metals, Inc., Greenville, South Carolina; R. L. Beatty, Manager of Ceramics: ARCO Metals is interested in assigning one of its staff to work part time at ORNL to participate in processing studies of incorporating silicon carbide whiskers into ceramic matrix composites.

As far as universities are concerned, our connections have always been extensive and I will merely illustrate their pervasiveness by calling to your attention the interactions of one group of the HTML, the high temperature materials synthesis group, with the academic community. This group has developed a number of new and novel techniques for the preparation of refractory materials and for the growth of often unique single crystals. ORNL scientists collaborate with outside scientists to use these special research samples. Additionally, we provide specimens to other institutions to facilitate their research. These policies result in the use of the ORNL high temperature research samples in numerous projects sponsored by DOE, NSF, and DOD. Such projects include research activities at various universities, other National Laboratories and, in some cases, industrial research centers.

Over 300 single and polycrystalline research samples of high temperature materials have been shared with approximately 70 research organizations in the past two years (see Appendix II for a selected list).

It may be instructive to recall the recent funding history of the HTML. It was included in the FY-82 budget and authorized by this Committee, but unfortunately not included in the FY-82 Appropriations Bill. In FY-83 the HTML was, I believe, included in the Senate Appropriations Bill (the authorization was still good from the previous year), but because of the continuing resolution that was eventually passed, the FY-83 funds were not forthcoming. Both in FY-82 and FY-83 the funds were to come from the Office of Basic Energy Sciences of the Department of Energy. In the DOE FY-84 submission, the HTML is to be funded in the Conservation Program.

As you might expect, the Oak Ridge National Laboratory has not been standing idle waiting for funding of the High Temperature Materials Laboratory before it engages in research in this vital area. We have in fact reoriented much of our basic materials sciences program and expanded our applied program in areas of high temperature materials. I can not, in a document this short, go into a complete description of our programs nor can I recount all the advances that have been made. Let me just describe a few of our salient developments in the field.

1. Development of $Ni_3$ aluminides. It has been known for a long time that nickel aluminide ($Ni_3Al$) is a very strong material. It was totally useless, however, for any practical application because it was brittle and could be produced only as a crumbling collection of small particles. During the past year C. T. Liu and collaborators at the Oak Ridge National Laboratory were successful in developing nickel aluminide in a workable normal metal with the following characteristics: 50% ductility, oxidation resistance of stainless steel, creep rates 20 times lower than stainless steel and three times lower than superalloys at 760°C, a yield strength that exceeds stainless steel and superalloys by factors of 7 and 3½, respectively, at elevated temperatures. This startling result

was obtained by the addition of boron (between 250 and 1000 ppm) to the nickel aluminide. Figures 1 and 2 present the data as they have been developed over the past year.

2. Toughened ceramics. We generally think of ceramics as brittle materials having low tensile strengths and poor resistance to fracture. In fact, these characteristics often limit the use of ceramics when their other properties make them very desirable (e.g., chemically inert, stable at high temperatures). This brittleness in ceramics is illustrated by their low fracture toughness ($K_{IC} \leq 5$ MPa · $m^{1/2}$) as compared to that of most metals (typically, $K_{IC} > 50$ MPa · $m^{1/2}$) - even brittle aluminum alloys ($K_{IC}$ = 15-20 MPa · $m^{1/2}$). ORNL researchers are developing much tougher ceramics by incorporating zirconia particles which exhibit a phase change under stress. This behavior imparts significantly greater toughness ($K_{IC} \sim 13$ MPa · $m^{1/2}$) to the high strength (>100,000 psi) ceramics. Obviously, such toughened ceramics have tremendous potential for high-stress applications (e.g., heat engines, cutting tools).

Figure 3 shows the properties of toughened alumina compared to glass, ceramic oxides, and carbides and nitrides.

3. Hydration of Beta-Alumina. Two groups of materials known as the beta- and beta"-aluminas have extraordinarily high ionic conductivities, which make them promising candidates as solid electrolytes in electrochemical devices. This high conductivity is partly a consequence of their crystal structure, which is characterized by thin loosely packed layers containing the mobile cations separated by thick densely packed layers containing oxygen and aluminum ions. But another consequence of their structure is that the beta-aluminas tend to absorb water. The absorption of water degrades the ionic conductivity of beta-alumina by as much as a factor of five and causes other undesirable

physical and chemical changes to occur. Recent research at ORNL has been directed toward understanding the reaction of the beta- and beta"-aluminas with water and the effect of water on the properties of these materials. To this end, the research has included studies of (1) the structure of the hydrated compounds, (2) the thermodynamics and (3) kinetics of the hydration reactions, (4) the behavior of water in the crystal lattice, and (5) the effect of water on ion transport.

The structure of hydrated lithium beta-alumina, Figure 4, was determined by neutron diffraction. The water molecules are located in the conduction plane and the protons form hydrogen bonds with oxygen ions in the layers above and below the plane.

In lithium beta-alumina, but apparently not in the other beta-aluminas, the water molecules dissociate to form $OH^-$ ions and hydrated proton species, $H^+(H_2O)_n$. This dissociation is believed to be the key step in the very rapid diffusion of water in this material, without requiring molecular diffusion, which would proceed much more slowly. The absorption of water decreases the conductivity of beta-alumina because the $H_2O$ molecules block the conduction pathways and they increase the activation energy for conduction as a result of the ion-dipole bonding between the molecules and the mobile cations. The absorption of water also causes an expansion of the lattice, thereby contributing to mechanical failure.

These illustrative examples provide a clear indication that high temperature materials research can be exceptionally productive. We have merely scratched the surface in this area of science and technology, and many new scientific insights of practical materials should be obtained in the future.

At Oak Ridge we would embed the High Temperature Materials Laboratory in a strong research establishment with a long tradition of successful research in materials, with the capability of designing new equipment, with a proven record of accomplishment in the development of alloys and ceramics, and in an organization that places high importance on interactions with industry and universities.

Appendix I

## HIGH TEMPERATURE MATERIALS LABORATORY INDUSTRIAL USERS COMMITTEE

Dr. Elihu F. Bradley - Chief Materials Engineer
United Technologies
71 Crestwood Road West
Hartford, Connecticut 06107

Dr. Arthur W. Dana, Jr. - Vice President
Fansteel Corporation
800 Market Street
Waukegan, Illinois 60085

Dr. R. F. Decker - Vice President for Research
Michigan Technological University
Houton, Michigan 49931

Dr. E. H. Kottcamp, Jr. - Senior Vice President
for Operations
Bethlehem Steel Corporation
Bethlehem, Pennsylvania 18016

Mr. W. D. Manly - Senior Vice President
Cabot Corporation
125 High Street
Boston, Massachusetts 02110

## Appendix II

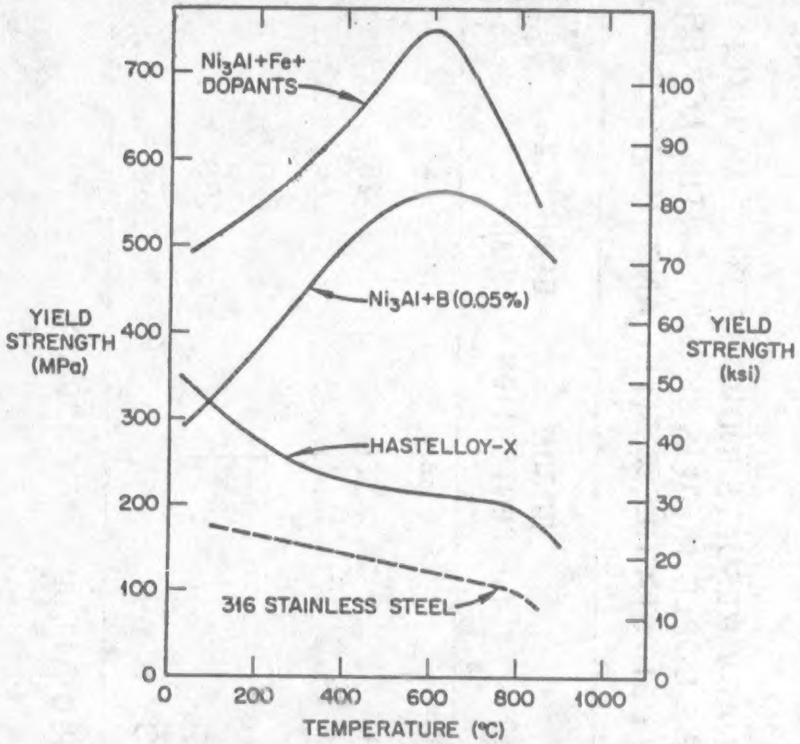
## EXAMPLES OF COLLABORATIVE AND COOPERATIVE EFFORTS INVOLVING THE USE OF SAMPLES FROM THE ORNL HIGH TEMPERATURE MATERIALS PROGRAM

| Organization and Individual Involved | Material | Type of Study |
|---|---|---|
| Cornell U.<br>(K. Krasst) | Nb | Thermal conductivity |
| Oklahoma State U.<br>(G. Summers) | MgO | Photoconductivity |
| Ohio State U.<br>(D. Readey) | CaO | Crystal characterization |
| Illinois Inst. of Tech.<br>(H. Weinstock) | MgO:Li | Thermal conductivity |
| U. of North Carolina<br>(D. Eisenberg) | MgO:Li | Electrical conductivity |
| M.I.T.<br>(W. Kingery) | MgO:Fe | Magnetic resonance |
| U. of Kansas<br>(W. Unruh) | MgO:K | Light scattering |
| U. of Illinois<br>(H. Stapleton) | CaO:Ca | Relaxation times |
| Virginia Polytechnic Inst.<br>(D. Hasselman) | MgO | Conductivity |
| Columbia U. | MgO:Li | Internal friction |
| University of Utah<br>(F. Lutz) | MgO:Li | Raman scattering |
| U. of Southern California<br>(J. Feinberg) | $KTaO_3$ | Laser interactions |

| Organization and Individual Involved | Material | Type of Study |
|---|---|---|
| Indiana U. (L. Chase) | $KTaO_3$ | Light scattering |
| Baylor U. (W. O. Milligan) | $LaPO_4$ | Crystal structures |
| U. of Tennessee (J. Thompson) | $KTaO_3$ | Superconductivity |
| Iowa State U. (J. Staudenmann) | $V_3Ge$ | Neutron scattering |
| Lawrence Berkeley Lab. (N. Edelstein) | $HfSiO_4$ | Optical studies |
| Brookhaven National Lab. (J. Axe) | $KTaO_3$ | Neutron scattering |
| Lawrence Livermore Lab. (W. Gourdin) | MgO:Li | Oxygen effects |
| Lincoln Labs (MIT) (A. Mooradian) | MgO:Ni | Laser fabrication |
| IBM Research Lab. (H. Höchli) | $KTaO_3$ | Ferroelectrics |
| Eastman Kodak Research Lab. (A. Wu) | MgO | Ceramics research |

ORNL-DWG 82-11996R

# THE YIELD STRENGTH OF $Ni_3Al$ ALLOYS INCREASES WITH TEMPERATURE AND REACHES A MAXIMUM AROUND 600°C



- BECAUSE OF THE INCREASE IN YIELD STRENGTH WITH TEMPERATURE, $Ni_3Al$ ALLOYS ARE MUCH STRONGER THAN COMMERCIAL ALLOYS AT ELEVATED TEMPERATURES.

ORNL WS-25060

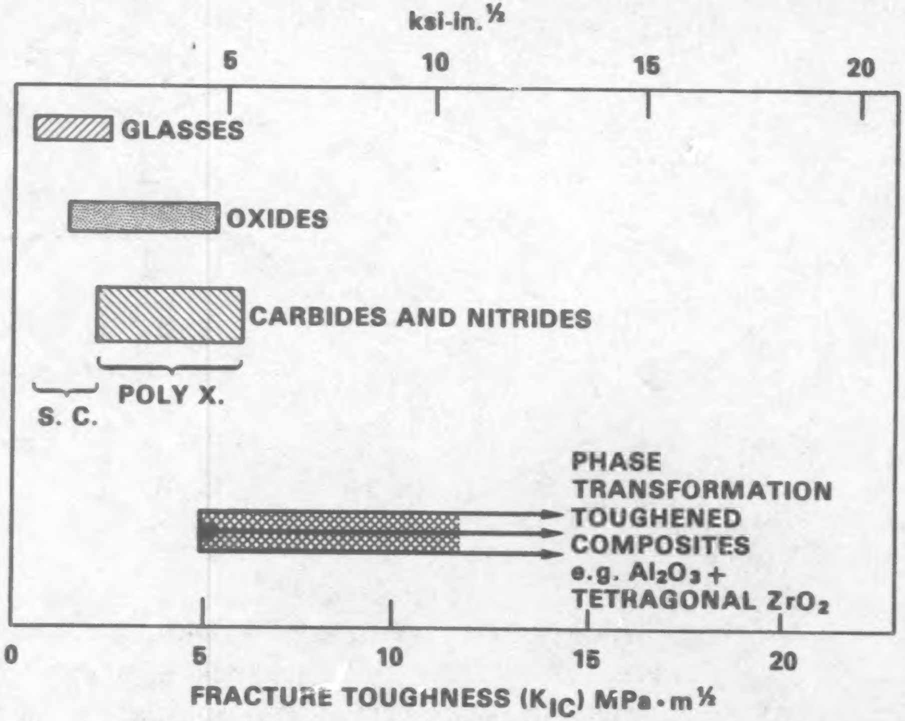
## PRELIMINARY RESULTS INDICATE THAT MINIMUM CREEP RATE OF B-DOPED $Ni_3Al$ IS SIGNIFICANTLY LOWER THAN TYPE 316 STAINLESS STEEL AND HASTELLOY X

| ALLOY | BORON CONCENTRATION | GRAIN SIZE ($\mu m$) | MINIMUM * CREEP RATE ($10^{-5}/h$) |
|---|---|---|---|
| $Ni_3Al$-A | 250 | 25 | 41.8 |
| $Ni_3Al$-B | 500 | 25 | 40.4 |
| $Ni_3Al$-C | 1000 | 25 | 39.3 |
| $Ni_3Al$-D | 1000 | 120 | 1.8 |
| 316 SS | – | ~100 | 854 |
| HASTELLOY X | – | ~150 | 132 |

*MEASURED AT 760°C AND 138 MPa

ORNL-DWG 83-8474A

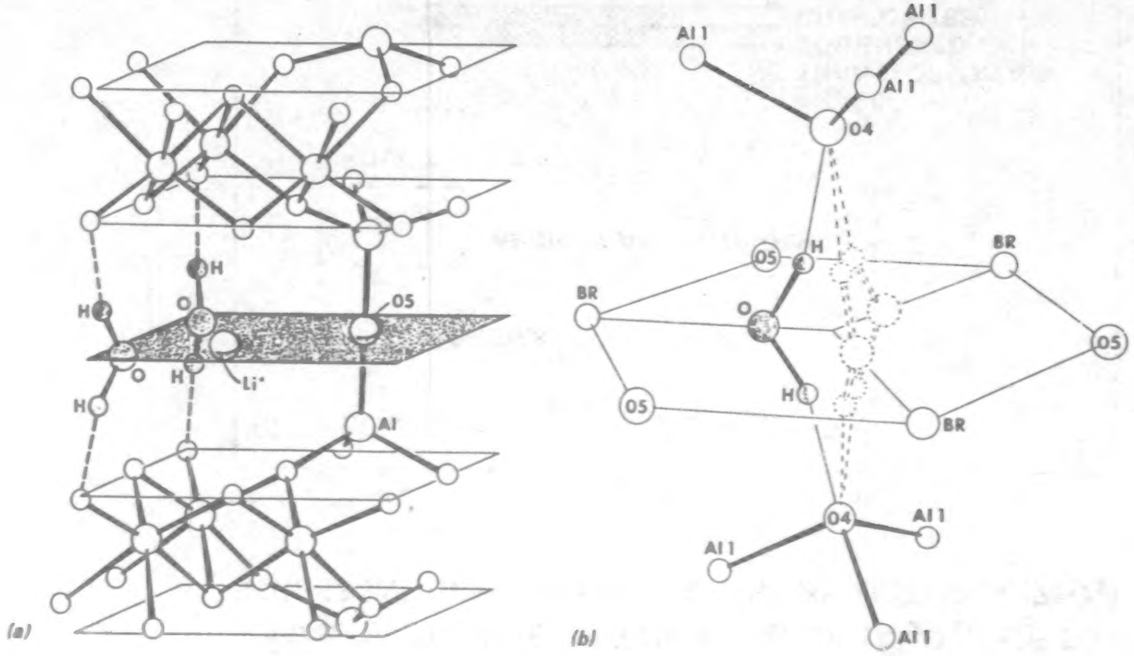
## 200-300% TOUGHENING OF ALUMINA ($Al_2O_3$) DUE TO STRESS-INDUCED PHASE CHANGE BY ZIRCONIA ($ZrO_2$)



2/1/83

ornl

SCHEMATIC VIEW OF THE CONDUCTION PLANE OF Li β-ALUMINA CONTAINING $H_2O$ MOLECULES. THE DIAGRAM ON THE RIGHT SIDE SHOWS THE DIFFUSION MECHANISM INVOLVING DISSOCIATION, DISPLACEMENT, AND RECOMBINATION.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(b)

Mr. **Fuqua**. Thank you very much, Dr. Zucker.

Now, Dr. Bjorken. Am I pronouncing that correctly? Like bureaucrat, but Bjorken. [Laughter.]

## STATEMENT OF DR. JAMES D. BJORKEN, SENIOR SCIENTIST, FERMI NATIONAL ACCELERATOR LABORATORY

Dr. **Bjorken**. Thank you, Mr. Chairman.

I first want to express the regrets of the Fermilab director, Dr. Leon Lederman, that he could not be here today. I must also express my own regrets in that sense that I can hardly match him. My name may sound like bureaucrat, but I am not one. I am not part of the laboratory administrative structure. Instead, I am a theoretical physicist.

In addition to the information about me that you have, I would add that I have been involved in national planning questions in high-energy physics. I served on the last three HEPAP subpanels on new facilities, the so-called Woods Hole committees and the Trilling committees of 1977, 1980, and 1981, and I am involved at Fermilab in questions of long-range planning.

My remarks here will address the state of high-energy physics in the context of the U.S. program and the Fermilab program and with, of course, fiscal year 1984 in mind.

Our business as high-energy physicists is to discover the most basic building blocks of matter and to understand the forces that hold them together. The progress in the last decade and a half has been extraordinary. A decade ago, only two of the four known forces then, electricity and gravity, were what we could call well understood. The other forces, which are known as the weak and the strong forces, really were not understood at all. There was chaos.

Now these latter two are believed to be understood as well as the electromagnetic and the gravitational force in the sense that the depth of understanding is at the same level as the depth of understanding, let us say, that is present in Einstein's general relativity. This is a profound accomplishment and a permanent one, and one that we can be very proud of, in that the U.S. high-energy physics program was a major contributor to that success.

There is also, in addition to a simple picture of the forces, a simple picture of building blocks which did not exist a decade ago. We have a kind of periodic table of basic building blocks which is on the second page of my written testimony, which, by the way, I would like to submit for the record.

When we came into the 1970's, of the 12 building blocks in that table, 7 were more or less on the record—even there, one could debate it—and 3 more were found during the 1970's due to major discoveries at SLAC, Brookhaven, Fermilab, and Cornell.

Our confidence in the pattern is such that, while the remaining two have not yet been discovered, there is every expectation that within the next decade they will be. This is the top quark and the tau-neutrino.

Now, some of the latest and most vital discoveries in our field have come from Europe. The most direct evidence of the correctness of our picture of the strong force first came from the DESY

Laboratory in Hamburg, Germany, with confirmation in the PEP facility at Stanford. This is the discovery of the gluon, the particle that mediates the strong force.

For the weak force, there is the absolutely superb result from Geneva, Switzerland, in the CERN Laboratory of the intermediate boson or W-particle which has been sought after for decades, convincing evidence for it having been found in that experiment.

Therefore, there is concern—concern is growing—that the leadership in high-energy physics might be rapidly moving to Europe. I think we must be prepared for that concern to intensify in the next couple of years as more European discoveries come in.

In the next year, with near certainty, we can expect the discovery, again at CERN, of the important companion of the W-particle called the Z. Also within the next year, the search in Europe, and only in Europe, for the building block called the top-quark will be greatly expanded, and I think there is quite a good chance—fair to good, anyway—that it will be discovered either at CERN or at the DESY Laboratory.

At least for the next 2 or 3 years, there is no U.S. facility that can compete in these particular searches with the European facilities. Therefore, that raises the question of whether or not the U.S. program has slipped to a position of inferiority.

I firmly believe that the answer to that question is no. We have seen this situation coming for some years, and the issue of whether to try to engage in a race for W and Z was raised at Fermilab, at least by 1979, and the laboratory decided then not to engage in a direct race. It was felt that to do so would impart unacceptable damage both to the ongoing program and to the long-range plans of the laboratory. In my mind, there is no question as to the correctness of that decision and to the basic strength of the existing plan.

What is this program at Fermilab? In addition to the original 400 GeV accelerator complex, which has been operating for a decade, it consists of three major new components, the energy saver, Tevatron II, and Tevatron I.

The energy saver, commonly known as the Tevatron, is a new accelerator ring, 4 miles around, in the same tunnel as the old one, of 1,200 superconducting magnets and other devices involving novel cryogenic technology. Because of the larger magnetic field in the superconducting magnets, this will more than double the output energy of the accelerator complex and make it indisputably the highest energy machine in the world throughout the 1980's.

There are great electrical power savings in this machine as well amounting to about 40 megawatts. This project required construction of a very large liquid helium plant, central helium liquefier, which is as large as any in the world, and it required the development of these superconducting magnets involving a large interaction with industrial vendors to produce the various components.

I am happy to report that the saver project is rapidly nearing completion. Almost all the magnets ordered, 90 percent, are installed now in the 4-mile tunnel. Half the ring is today cold, with 4 degrees, Celsius, of absolute zero, leak-tight, and has been powered to between 600 and 700 amperes successfully.

The central helium liquefier is working smoothly, and the remaining installation is proceeding very well. It should be finished this spring, with every expectation that we will be able to commission the machine and start beam tests during the summer.

I think this is a very major step forward because all future proton accelerators will necessarily depend on the superconducting technology, and the successful commissioning of the saver will represent a major advance in accelerator science.

Tevatron II is a construction project whose purpose is to upgrade the delivery system of the protons, the high energy, 1,000 GeV protons, from the energy saver to the many experimental facilities, so that the higher energy of the Saver can be fully exploited in the experimental program.

Many beam lines have to be upgraded in a way that they can operate simultaneously and support the very large experimental program which comprises at least half of the total national high-energy physics program. About 600 professors and students from 70 institutions are involved as of this date.

The Tevatron II project is well underway, proceeding satisfactorily, within budget, and on schedule. We hope that that will be finished by the end of 1985.

The scientific program for this fixed-target capability is very rich and very deep. There will be searches for the tau-neutrino and other neutrinos, for that matter, which will be undertaken. I believe that, in the long run, this facility will be especially powerful in studying in detail the strong and weak force as well as the matter containing two of the new quarks; namely, the so-called charm quark and the bottom quark.

It is well and good to make the initial headline-winning discoveries of these particles and phenomena, but to really understand them requires a lot of detailed, hard work, and Tevatron II is a good facility which will support that kind of programmatic research.

Finally, the goal of the Tevatron I project is to turn the energy saver into a proton-antiproton storage ring. This is a device which is very similar in concept to what was built at CERN to find the W-particle. Counterrotating beams of 1,000 GeV protons and antiprotons are made to collide, allowing unprecedentedly large releases of energy per collision. This energy release is about 50 times as much as what one can get when you send a beam of protons on a fixed target.

The project consists not only of modifications to the saver ring but also of construction of an antiproton source and of a massive detector to observe the products of the proton-antiproton collisions.

The construction of this project is under way. The construction of the detector enclosure, which is a very large building, mostly underground, is well under way, again ahead of schedule. Construction of the components of the detector is under way and of the antiproton source is starting up.

It is expected that this physics program will begin in late 1985 or early 1986, and we are eager to get going on it. Studies are under way to try to accelerate this schedule. It is too early to know how much we might succeed in doing it.

The Tevatron I project will far surpass in scope what now exists in the highly successful CERN facility. The collision energy will be four times higher; the rate of proton-antiproton collision at least 10 times higher than what now exists at CERN. The antiproton source is of advanced design, one which is built on the experience at CERN and which goes a step beyond the CERN design.

The scientific goals of Tevatron I also go a step beyond the W and the Z. If the top quark is too heavy to be found in the present searches in Europe, Tevatron will be able to search at even higher masses for that particle.

There may be a fourth row in the periodic table which I showed you consisting of heavy particles waiting to be discovered, or the building blocks which we think today are fundamental may themselves contain constituents as has been the case in the past so often.

It is for these reasons that I here describe that the immediate race for W and Z was foregone in order to have a really first-class facility in the long run, the long run being 1985 or 1986.

Beyond 1985 or 1986, the issue of what lies beyond the energy scale and the W and the Z will probably dominate the late eighties and the early nineties. The Tevatron I collider will be the first step into that energy domain. But I believe a strong initiative which goes to energies still beyond the 1,000 GeV of the Energy Saver is required. By the middle 1990's, Europe should have a capability for doing this.

The large 17-mile circular tunnel, four times the size of the present Fermilab tunnel, being built for the LEP electron-positron ring will eventually allow a proton-antiproton ring four times larger than the Tevatron. We don't know if and when that machine will come to pass.

What should the United States do? I don't know myself. A sharp policy is needed, and it is needed soon, and at Fermilab we are, together with the national community, exploring what the best strategy might be. This program must be cost effective, avoid duplication of effort as much as possible, but, first and foremost, it has to produce firstclass science and science which will be first class in the early 1990's.

I think these long-range issues should not directly impact the problem of fiscal year 1984 and what is in front of us right now. For that, I would say the Fermilab priorities are as follows: First of all, we have to get the saver going and operating year round. We want to complete the Tevatron II construction in a timely way so that the Tevatron can be utilized most effectively for a large community of users.

The program needs an appropriate level of support for the university community of physicists who will do the experiments. We need adequate support of the Tevatron I proton-antiproton collider construction to allow commissioning at least by 1986.

We cannot forget the long-range future, and therefore there is a need to pursue R&D toward the next step as well.

Thank you, Mr. Chairman, for the opportunity to speak here.

[The prepared statement of Dr. Bjorken follows:]

Statement for House Committee on
Science & Technology Hearings
Subcommittee on Energy Development and Applications
March 3, 1983

## The Fermilab High Energy Physics Program

James D. Bjorken*

Dr. Leon Lederman, the Fermilab Director, has asked me to express his regrets at not being able to be here today. I express my regrets as well; I can hardly match his elan and authoritative expertise. , I am a theoretical physicist, not an administrator. I am not even an experimenter turned laboratory manager. I worked for many years at the Stanford Linear Accelerator Center (SLAC) and in 1979 moved to Fermilab. Corridors of power are not my natural habitat.

My remarks will address the status of high energy physics in the context of the U.S. program and the Fermilab program with, of course, FY 84 in mind.

The business of high energy physics is to discover the most basic building blocks of matter and to understand the forces which make them stick together. A decade ago, only two of the four known forces - electricity and gravity - were understood well. The other forces - the "weak" and the "strong" forces - were not understood at all. Now, the latter two are believed to be understood at a level as basic as the other two - although there is much detailed understanding still to come. The U.S. High Energy Physics Program has been a major contributor to this success, and there is no question that the public investment has been amply repaid, if only for this accomplishment. It is profound and permanent, in the same sense as Einstein's general relativity is profound and permanent.

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* See Appendix I

There is also a simple picture of the building blocks which did not exist a decade ago. There is now believed to be at least 12 types of building blocks which fit neatly into a "periodic table." Both the number of entries and their names are simple in comparison to their chemical counterparts:

| | | | |
|---|---|---|---|
| up | down | electron | electron-neutrino |
| charm | strange | muon | mu-neutrino |
| (top) | bottom | tau | (tau-neutrino) |

The three underlined entries emerged in the last decade, thanks to major discoveries at SLAC, Brookhaven, Fermilab and Cornell. The two entries in parentheses are not yet found. But the odds are favorable that they will be found in the next decade.

Some of the latest and most vital discoveries have come from Europe. Very direct evidence of the correctness of this picture of the "strong" force has come from the DESY Laboratory in Hamburg, Germany (the discovery of the "gluon"). For the weak force, there is the superb, very recent result from the CERN Laboratory in Geneva, Switzerland, providing direct evidence for the long-sought "intermediate boson" or W-particle. Thus, concern is growing that the leadership in high-energy physics is rapidly moving to Europe. We can expect this concern to intensify, as more European discoveries come in. In the next year, we can expect with near-certainty the discovery at CERN of the important companion of the W-particle called the Z. Also, within the next year, the search - in Europe, and only in Europe - for the building block called top-quark will be greatly expanded, and there is a fair-to-good chance it will be discovered. For at least the next two or three years, no U.S. facility can compete in these searches with the European facilities. Does this mean that the U.S. program has slipped to a position of inferiority?

I believe the answer is no. The issue of the "race for the W and Z" was raised at Fermilab about 1979, and the Laboratory decided then not to engage in the race. It was felt that to do so would impart unacceptable damage both to the ongoing program and to the long-range plans of the Laboratory. In my mind, there is no question as to the correctness of that decision, and of the basic strength of the existing plan.

What is the Fermilab program? In addition to the original 400 GeV accelerator complex, it consists of three major new components: the Energy Saver, Tevatron II and Tevatron I.

The Energy Saver (or Tevatron) is a new accelerator ring of 1200 superconducting magnets and other devices incorporating novel cryogenic technology. This will more than double the output energy of the accelerator complex and will make Fermilab indisputably the highest energy accelerator in the world throughout the 1980's. Construction started in 1979 and was based upon an R&D program that began in 1973. When cooled to the temperature of liquid helium ($4^{o}K$), superconducting magnets are capable of achieving far more powerful magnetic fields (in this case two and one-half times stronger than the conventional iron-copper magnets) and of sustaining this without the power dissipation of conventional conductors. The electrical power saving amounts to about 40 megawatts. In the cost optimized design, the new accelerator is being assembled in the same subterranean tunnel as the old machine, reaching an energy of 1000 GeV (Billion electron Volts).

The project required the construction of a unique liquid helium plant (central helium liquefier) which more than doubles the world's capacity to produce this fluid. During the R&D and construction period there has been a close and productive interaction with industrial vendors to produce the various components of the new accelerator. Some of the non-standard components were the superconducting wire, superconducting cable, special stainless steels, compressors and engines for assembling the helium refrigeration system.

The Energy Saver construction project is rapidly nearing completion. More than two thirds of the magnets have been installed in the four-mile circular tunnel, and at present about half of the ring has been leak-tested and cooled to the temperature of liquid helium. The central helium liquefier is working smoothly. The remaining installation is proceeding well and should be finished this spring, with commissioning of the machine during spring and summer. Low temperature technology is especially unforgiving, and there may be delays. But resumption of physics research is planned for autumn. All future proton accelerators will necessarily depend on superconducting technology, and successful commissioning of the Energy Saver will represent a very major advance in accelerator science and technology.

Tevatron II is a construction project whose purpose is to upgrade the delivery system of protons from the Energy Saver to the many experimental facilities so that the higher energy of the Energy Saver can be fully exploited. Fifteen beam lines, many of which are capable of operating simultaneously, will transport a rich variety of secondary particles to targets, where the research will be carried out by the bulk of U.S. university scientists in this field. About 600 professors and students from 70 institutions are involved as of this date. The Tevatron II construction is well underway and proceeding satisfactorily. The beam lines are upgraded in a phased schedule which will see all beam lines improved by the end of 1985.

This "fixed target" capability is the backbone of the U.S. high energy physics program. With an energy increase from 400 GeV to 1000 GeV, the scientific gains will be very great. Searches for the tau-neutrino (and other neutrinos) will be undertaken. Especially valuable will be a greatly increased capability to study in detail the strong and weak force as well as matter containing "charm" and "bottom" quarks. It is not enough just to make headline-winning discoveries of new particles. Their properties must be studied in depth to really know what they are and whether they conform to our notions of what we think they are.

The goal of the Tevatron I project, started in FY '81 is to turn the Energy Saver into a proton-antiproton storage ring. This is a device very similar in concept to what was built at CERN to find the W-particle. Counter-rotating beams of 1000 GeV protons and antiprotons are made to collide, allowing unprecedentedly large releases of energy per collision. The total energy release is 2000 GeV, about 50 times as much as when 1000 GeV protons impact upon a fixed target. The project consists not only of modifications to the Energy Saver ring, but also of construction of an antiproton source and of a massive detector to observe the products of the proton-antiproton collisions.

Antiprotons are produced by employing the "old accelerator" to generate collisions. The antiprotons are collected, stored and accumulated in a series of magnetic rings. A sophisticated technique of "cooling" enables the storage of large numbers of these exotic particles. After several hours of accumulation, antiprotons are injected into the accelerator where, because of their negative electric charge, they rotate in the opposite sense to protons. Bunches of both particles are accelerated simultaneously to the 1000 GeV limit and then they are permitted to engage in head-on collisions. The use of antiprotons permits the observation of colliding beams without the need of two rings, achieving considerable economies. Construction of the detector building, the detector, and the antiproton source are underway. It is expected that the physics program will begin in late 1985 or early 1986. Studies are underway to try to accelerate this schedule.

The Tevatron I project will far surpass in scope that of the now existing, highly successful CERN facility. The collision energy will be 4 times higher and the rate of proton- antiproton collision 10 times higher. The antiproton source is of advanced design, one which builds upon CERN's experience and goes a step beyond it. The scientific goals of Tevatron I go a step beyond the W and the Z. If the aforementioned European top quark search fails, Tevatron I will be able to extend it. There may be a fourth row to the "periodic table" or other heavier particles waiting to be discovered. The building blocks we today regard as basic may themselves contain constituents.

The issue of what lies beyond the energy scale of W and Z will probably dominate the late 1980's and the 1990's. The Tevatron I collider will be the first step into that energy domain. But a strong initiative which goes to energies beyond the 1000 GeV of the Energy Saver is required. By the middle 1990's, Europe should have a capability of doing this. The large 17-mile circular tunnel being built for the LEP electron-positron ring will eventually allow a proton-antiproton ring 4 times larger than the Tevatron. What should the U.S. do? A sharp policy is needed and needed soon. Fermilab is, together with the national community, exploring what the best strategy might be. The program should be cost-effective, avoid duplication of effort and, most importantly, produce first-class science.

These long-range issues should not directly impact the problem of FY 84. For that, the Fermilab priorities are as follows:

1. Efficient, year-round operation of the Energy Saver/Tevatron;
2. Completion of the Tevatron II construction so that the Tevatron can be utilized in a cost-effective way;
3. An appropriate level of support for the university community of physicists who will do the experiments;
4. Adequate support of the Tevatron I proton-antiproton collider construction to allow commissioning by 1986.
5. The additional wherewithal to pursue R&D towards the "next step."

The President's Budget for FY '84, if enacted, is just about enough to achieve these objectives.

Thank you for this opportunity to be here today.

Appendix I

## JAMES D. BJORKEN

### Date and Place of Birth

22 June 1934, Chicago, Illinois

### Degrees

B. S. -- Massachusetts Institute of Technology, 1956

Ph.D. -- Stanford University, 1959

### Positions Held

Research Associate, Stanford University, 1960
Assistant Professor, Stanford University, 1961-63
Associate Professor, Stanford University, 1963-67
Professor, Stanford University (SLAC), 1967-79

Senior Physicist, Fermi National Accelerator Laboratory, 1979 - date

### Honors and Awards

Recipient of the 1972 Dannie Heineman Prize, presented by the American Physical Society, for outstanding publications in the field of mathematical physics

Recipient of the E. O. Lawrence Award, presented by the U.S. Department of Energy, 1978

Elected to the National Academy of Science, 1973

Elected Fellow of the American Academy of Arts and Sciences, 1974

### Author of

Relativistic Quantum Mechanics (with S. D. Drell), International Series in Pure and Applied Physics. McGraw-Hill Book Company, 1964

About 100 scientific papers on theoretical particle physics and accelerator science

**Mr. Fuqua.** Thank you very much.

Dr. Richter, we are happy to have you here again in the capacity as a witness. You were here a couple of years ago, I believe.

**Dr. Richter.** Yes, sir, I was.

**Mr. Fuqua.** We are pleased to have you back again. You may proceed.

## STATEMENT OF DR. BURTON RICHTER, TECHNICAL DIRECTOR, STANFORD LINEAR ACCELERATOR CENTER

**Dr. Richter.** I welcome the opportunity to testify again before this committee. I ask that a more extended version of my remarks be included in the record.

**Mr. Fuqua.** Yes, we will do that, and if you wish to summarize, please proceed.

**Dr. Richter.** I am here to tell you about the proposed new electron-positron colliding beam facility called the SLAC linear collider, or SLC for short. This new facility is both an extension of parameters of previous electron-positron colliders and it is a departure in technology from them.

The extension of parameters is an increase in energy by about a factor of 2.5 over the highest energy machines now running, and this increase in parameters allows a whole new class of experiments to be done with this kind of machine.

The departure in technology is in the construction of a new kind of colliding beam device that promises to reduce sharply the cost of reaching high energies over the cost of conventional colliders and, when we need them, to allow the construction of much higher energy machines at costs which we can ask the Government for without blushing.

Let me make a few remarks about physics and where this particular project fits into the exciting things that are going on in high-energy physics now. Dr. Bjorken told you something of new developments in theory of the weak interactions and the strong interactions. I think he put the case a little bit strongly. I don't think the weak and strong interactions are understood in the sense that electromagnetism and gravity are, but I am an experimenter, and experimenters are supposed to be skeptical, and it is the job of the experimenters to keep pushing and testing the theories that come from the theorists like Dr. Bjorken.

In this testing, the SLC has a very important role. The new theory of the weak interactions, the new unified theory of the electric and weak interactions, has become very popular and has received strong support experimentally, particularly with recent results from CERN on the discovery of the W particle, one of the two special new particles that are required in that theory.

There is another particle that is required, the neutral member of this family called the Z, and that will also probably be found at CERN in the next few years.

But to really understand the theory in depth and to really know whether this is right, one must study in detail what it predicts, and one studies that in detail by studying the decay processes of this Z particle.

The SLC is designed to that, and the CERN facility which will probably find it, I do not believe can do it. The rates at that facility will be on the order of 10 to 100 per year. The rates at the SLC are so much higher than that that a few minutes of running at the SLC are equivalent to about a year of running at the CERN proton-antiproton collider.

So, by looking at large numbers of the decay processes of these particles, studying what they decay into and how these particles are related to the Z, one gets very detailed information about the neutral interaction. I hope by that we can find out whether we are really on the right track.

We may even find some information about the strong interactions by finding some of the special particles that are required in the strong interaction theories, for those, too, can come from the decay of the Z.

The physics community strongly supports the construction of the SLC. The Trilling subcommittee of the high-energy physics advisory panel, in its report last year, recommended the construction of the SLC on a 3-year time schedule as a high-priority item.

The European physics community also feels that experiments with electrons and positrons at this energy are very important, for they are now spending a great deal of money on what I might call an old-technology collider facility to reach the same energy. This is the great machine called LEP.

Let me turn now to the machine itself. As I mentioned earlier, it is a new departure in accelerator technology. Before describing the machine, it might be useful to tell you why a new departure is, I believe, necessary now.

The old technology is that of the colliding beam storage rings. The first of these machines was started in 1958, and I am proud to have been part of the group that started that and built that facility. The newest and largest was started last year, the machine called LEP at CERN. It is designed to reach 100 GeV in the center of mass, the same energy at the SLC, and scheduled to be finished in 1988.

In the 25 years since the first machine was started, the radii of the machines have grown 5,000-fold; the energy of these machines has grown 100-fold. The scaling law for the size and cost of these machines with energy is now well known. One does not have to design a storage ring in detail to know what it will cost, and one knows that storage rings increase in size and cost as the square of the energy.

The budget for the biggest, and what I think is the last, of the storage rings, the LEP machine, is 900 million Swiss francs, plus costs for staff. That translates to something like $500 million. If we ever wanted a machine 3 times the size of that, it would cost us 10 times as much money. Ten times $500 million is a number so large that while the machine might be technically feasible, I question its fiscal feasibility.

So we need a new system. That is not new in high-energy physics. We have needed new systems before. If you tried to extrapolate Lawrence's early cyclotrons to the energy of the Fermilab synchrotron, you would have a cost so unbelievable that it would dwarf the defense budget.

So we keep on inventing new kinds of accelerators, and those inventions allow us to keep on going up in energy, probing deeper and deeper into matter, and at a cost which the Congress is willing to pay.

The SLC is a variant of a new kind of machine called the linear collider. Linear colliders have a cost-scaling law which is proportional to the first power of the energy.

There is a sketch of a linear collider on the next-to-last page of my written testimony. If you don't have it, you can just think of the linear collider as looking something like a tennis racket. The handle of the tennis racket is the heart of this project. It is the Stanford linear accelerator, the 2-mile-long electron accelerator which is to be upgraded in energy to go from its present energy of 32 GeV to an energy of 50 GeV. The wooden frame of the head of the tennis racket represents a collection of tunnels, 2.3 kilometers of tunnels, filled with small magnets which guide bunches of electrons and positrons accelerated in the linac around in opposite directions to collide at one point directly opposite the handle.

The electromagnetic fields associated with these bunches are so strong that when they collide, the bunches are violently disrupted; so badly disrupted that they cannot be put back together again. One disposes of them and starts all over again.

In addition to these main components, of course, the project requires the construction of complex apparatus for the production of electrons and positrons of a necessary size and intensity to be injected into the linac.

Now, the repetition rate of this SLC is only about one one-thousandth the collision frequency of a typical large storage ring. But the particle densities in the beams are enormous relative to those in the storage ring. They are about 50,000 times the density.

The beam is about one-tenth the diameter of a human hair, and the density approaches that of a room temperature gas which, for any accelerator, is a very, very high density indeed. The small beam size and high density make up for the low repetition rate, and so this way you get performance comparable to the performance of storage rings.

The facility's total estimated cost is $112 million. It is divided roughly, one-third into conventional facilities, buildings, tunnels, AC power distribution, and so forth, and two-thirds into technical components.

We have requested $40 million in new obligational authority in the first year of construction, and about that level of first-year funding is required if we are to complete the project on a 3-year construction schedule.

The reason for the high first-year funding is that the project is paced by its conventional facilities, and if we are to do it in 3 years, we have to get the tunnels under contract in the first year.

Because of the high priority put on this project by the physics community, the Department of Energy has given us preliminary engineering and design funds to allow the design work on the tunnel to be done in advance of project authorization.

Our plans call for letting the tunnel contract as soon as the funds are available. In the event that Congress and the administration do agree that Federal construction projects should be accelerated to aid in job creation, we could get this whole thing contracted in this fiscal year.

Let me briefly summarize as follows: The SLC is an innovative project that represents a significant advance in high-energy physics research and a novel development in the technology of accelerator construction. The advance in research is the advance in energy that allows a detailed testing of our theories of the weak interaction, and the advance in accelerator technology hopes to sharply reduce the cost of big machines.

An intensive R&D program has been pursued over the last 3 years, and this R&D program leads us to believe that we can build the machine on this 3-year time schedule with very high confidence.

Given the very great promise of the project for the future of the U.S. program in high-energy physics and the readiness of the SLAC staff to undertake the work, we hope that this committee will act favorably on this item.

Thank you.

[The prepared statement of Dr. Richter follows:]

# STATEMENT BEFORE THE SUBCOMMITTEE ON ENERGY DEVELOPMENT AND APPLICATIONS

House Committee on Science and Technology

Burton Richter
March 3, 1983

## INTRODUCTION

I welcome the opportunity to testify to this Committee on the proposed new electron-positron colliding-beam machine, the SLAC Linear Collider (SLC). This facility is both an extension of parameters of previous electron-positron colliders and a departure in technology from them. The extension of parameters is an increase in energy by about a factor of 2.5 over the highest-energy electron-positron machines now operating (PETRA at Hamburg, West Germany and PEP at SLAC). This increase in energy will allow a new cycle of experiments aimed at studying the weak interaction and the new particles that are believed to be associated with that interaction.

The departure in technology is in the construction of a new kind of colliding-beam machine that promises to reduce sharply the cost of reaching high energies over the cost of conventional colliders and also to allow the construction of much-higher-energy machines when needed at a cost that the physics community could request from the government without blushing.

## PHYSICS

This is a particularly vital time in high-energy physics. We believe we are on the verge of developing a new synthesis wherein all the physical universe can be described by a handful of fundamental entities (the quarks and leptons) and by one or two elementary forces. This new view has developed during the past decade. Two great experiments in 1973 and 1974 started this new cycle of synthesis. In 1973, physicists at CERN found that so-called weak force (then thought responsibility only for radioactive decay) was effective in systems without electrical charge as well as in systems with it. In 1974, physicists at SLAC and BNL found evidence for a massive particle three times heavier than the proton that was unstable but relatively long-lived. The SLAC group showed in subsequent experiments that this was conclusive evidence for a new basic building block of matter.

These two experimental developments pointed toward which of a host of possible theories was the "right" one. During the past ten years many experimental discoveries and their theoretical interpretation have combined to make most physicists think that the dream of being able to describe the four basic forces of nature as different manifestations of the same force may finally be within reach.

A decade ago the basic forces were thought to be these: (1) electromagnetic, (2) weak, (3) strong, and (4) gravitational. The first break-through was the gauge theory of the electro-weak interaction that unified the first two forces. Glashow, Salam and Weinberg were awarded the Nobel Prize for their pioneering work in the development of this theory. Recently experiments at the 540-GeV proton-antiproton collider at CERN have found one of the two special particles predicted by this theory at about the correct mass. Now most physicists would agree that the basic elements of this, the simplest of the electro-weak theories, are probably correct.

Work has been going on for some time on a more difficult unification, that of the strong force with the electro-weak force. These models, known as Grand Unified Theories (GUTs), predict many new phenomena. In particular, the simplest version of these theories predicts that the proton is unstable, though it is predicted to have an extremely long lifetime. Recent experiments have set a lower bound on the proton lifetime, longer than that predicted by this model; and so the simplest of the GUTs is probably wrong.

Work on the most difficult unification, that involving gravity, is still in such a primitive state that the theory doesn't predict anything yet, so it is invulnerable to the probing of experimenters.

The SLC project fits into this emerging synthesis as the machine that can test the electroweak theory in detail, and that will allow a hunt for some of the exotic particles required in the strong-electroweak unification. The electroweak theory predicts a neutral particle with a mass of about 94 GeV (the Z). This particle might also be discovered at CERN in the next year or so, but the expected rate of production of such particles in the CERN facility is so low (about 10 per year) that little in the way of detailed study can be done. In contrast to the low rate in proton-antiproton collisions, the SLC's design intensity is such that the expected production rate of Z particles is 10 per minute, i.e., one minute of experimentation at the SLC is equivalent to one year at the CERN proton-antiproton facility. Further, this production rate is much less subject to uncertainties in the strong-interaction physics which confuse the interpretation of events in proton colliders. Also, the background in electron-positron collisions is many orders of magnitude lower than in proton interactions, and thus separating the wheat from the chaff is considerably easier.

When the Z decays,it can decay into all of the other fundamental entities that have masses smaller than that of the Z. It is through the study of these decays that the electroweak model can be tested in depth. The contributions to the development of strong interaction models are made by searching in the decay fragments of the Z for the new particles required in some of these models. It is interesting to note that all particles that have strong interactions also have electroweak interactions, although the converse is not true. As the Z decays it can produce any of the things taking part in the strong interaction as long as their masses are less than that of the Z. Experiments at the SLC may, thus, point the way to a better strong interaction theory.

The physics community strongly supports the construction of the SLC. The Trilling Subcommittee of the High Energy Physics Advisory Panel (HEPAP) in its report last year recommended the construction of the SLC on a three-year time schedule as a high-priority project. The European physics community also feels that experimentation in electron-positron collisions at this new energy is vital, for they are now spending a great deal of money on an "old technology" colliding-beam machine called LEP.

## THE MACHINE

As I mentioned earlier, the SLC is a new departure in accelerator technology. Before describing the machine, it might be useful to tell you why a new departure is necessary. The "old" technology is that of the colliding-beams storage rings, wherein beams of electrons and positrons circulate in opposite directions in a single magnetic guide field and the same particle bunches collide with each other many times per second. The first of these electron storage rings, the Princeton-Stanford machine, was begun in 1958, and I am proud to have taken part in the design, construction, and use of that facility. A few years ago, two new big brothers of this first machine were completed at SLAC and at DESY in West Germany, and these machines are now being exploited by a total of about 800 physicists. Construction of the largest of the electron storage rings, the LEP project at CERN, has begun and if all goes well that facility will be operational in 1988 at an energy of 100 GeV in the center of mass, the same as the design energy of the SLC.

In the 25 years since the start of the Princeton-Stanford project, the radii of these machines have grown 5000-fold, and the energy of the machines has grown 100-fold. The scaling law of cost vs energy of electron storage rings is well-known and it is this scaling law that will make the LEP project the largest and last of the high-energy electron storage rings.

The problem is that electrons radiate energy when they are forced to travel in a circle, and the power required to keep a beam going increases very rapidly as the beam energy increases.

The accelerator builder can take such information as the power requirement vs. energy, costs of magnets and tunnels, etc., and derive an equation that minimizes the cost vs. energy of a machine fairly independently of the details of the machine design. The scaling law for electron storage rings is that the size and the cost are proportional to the square of the energy, i.e., doubling the energy increases the costs by four.

LEP is budgeted at Sw.Fr. 900 million (1982 francs), excluding staff costs. A machine three times the size would cost ten times as much, and I conclude that the fiscal feasibility of such a follow-on project is very much in doubt.

The SLC is a variant of a class of machines called linear colliders, and the scaling laws for these machines are such as to make the costs proportional to the first power of the energy.

A sketch of the SLC project is shown in Fig. 1. The heart of the project is the existing SLAC two-mile linear electron accelerator. The energy of the linac will be increased to 50 GeV as part of this project, and the linac will be used to simultaneously accelerate two intense bunches of particles, one of electrons and one of positrons.

At the end of the accelerator the two bunches are separated by a magnetic field, and each enters its own branch of a tunnel of total length 2.3 kilometers. In the tunnel, small conventional magnets guide the particles through the opposite arcs and bring them to a special region containing the final focusing equipment. The final focus system reduces the size of each bunch to a diameter of about 3 microns and makes the bunches collide at the center of an underground vault where the experiments are performed.

The electromagnetic fields associated with these bunches at the collision point are substantial, and in the brief moment of collision the bunches are so badly disrupted that they cannot be reused. The spent beams are then guided to special beam dumps where their energy is dissipated. The cycle repeats 180 times per second.

In addition to these main components, the project requires the construction of some complex apparatus for the production of electron and positron beams of the necessary intensity and size to be injected into the linac at the start of each cycle.

The repetition rate of the SLC is only about one - one thousandth of the collision frequency in a typical large storage ring, but the particle density at the collision point is relatively enormous - about 50,000 times the density in a storage ring. The beam diameter in the SLC is about one-tenth that of a human hair, and the density approaches that of a room temperature gas. The small beam size and high density makes up for the low repetition rate compared to storage rings, and the performance of the two types of machines is expected to be comparable.

There are two new issues of accelerator physics associated with the SLC. The first involves the interaction of the intense bunches with the structure of the linac itself. This interaction if not properly controlled tends to increase the size of the bunches beyond that required for good performance at the collision point. We have been doing both theoretical and experimental work on this problem for the past few years. Our experiments have confirmed the theoretical calculations to an accuracy sufficient for us to have confidence that this problem can be controlled. The second new issue is in the interaction of the bunches at the collision point. This interaction is much stronger than the corresponding interaction in electron storage rings. Calculations indicate that this problem can be controlled and that indeed the beam-beam interaction can actually increase the effective intensity of a linear collider.

There are, of course, all of the standard problems in building any large high-technology project. For example, we have to make 1000 magnets to accurate tolerances, electron and positron sources, computer control systems, etc. We have been addressing all these problems in an intense R&D program over the past few years and we now believe we are ready to proceed to construction.

The facility's total estimated cost is $112 million, divided roughly one-third to conventional facilities and two-thirds to technical components. We have requested $40 million in new obligational authority in the first year of construction, and about that level of first-year funding is required if we are to complete the project on the three-year construction schedule. The reason for this is that the project schedule is paced by the construction schedule of the tunnels. Thus, the construction contract for the tunnels and major procurements of such things as steel for magnets must be made early in the project.

Because of the high priority put on this project by the physics community, the Department of Energy has given us Preliminary Engineering and Design funds to allow the design work on the tunnel to be done in advance of project authorization. Our plans call for letting this contract as soon as funds are available. In the event Congress and the Administration do agree that Federal construction projects should be accelerated to aid in job creation, we could let the tunnel contract in this fiscal year.

## SUMMARY

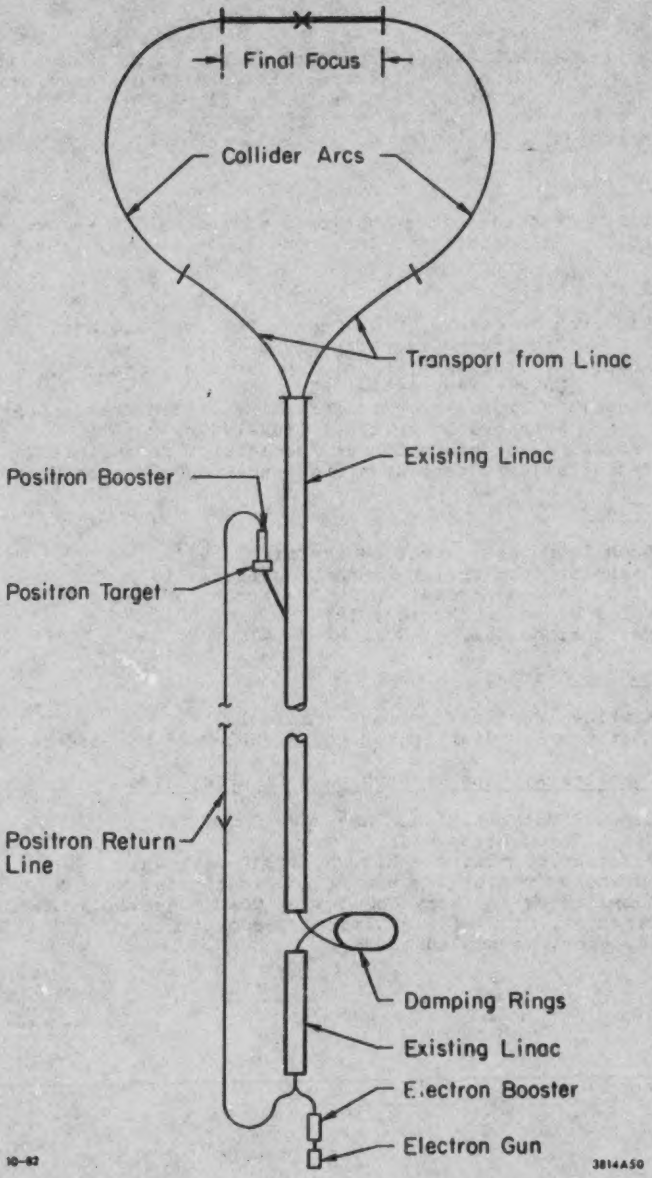
The SLC is an innovative project that represents both a significant advance in high energy physics research and a novel development in the technology of accelerator construction.

The physics program carried out with this new facility will make important contributions to our understanding of the basic structure of matter and the forces of nature.

The accelerator technology developed in the project should markedly reduce the unit cost of future high energy electron colliders and may also have application to proton machines.

An intensive R&D program over the last three years, together with PE&D funding during the past two years, has made the project ready for a fast construction schedule. This schedule is paced by the conventional facilities and thus requires large first-year funding to maintain it.

Given the great promise of this project for the future U.S. program in high energy physics, and the readiness of the SLAC staff to undertake the work, we hope this Committee will act favorably.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Positron Booster
Positron Target
Positron Return Line
Damping Rings
Existing Linac
Electron Booster
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# CURRICULUM VITAE

Burton Richter - Paul Pigott Professor in the Physical Sciences;
Technical Director, Stanford Linear Accelerator
Center, Stanford University

Born - March 22, 1931

## Education

B.S. - Massachusetts Institute of Technology - June, 1952
Ph.D. - Massachusetts Institute of Technology - Sept. 1956

## Employment

Research Associate, High Energy Physics Laboratory, Stanford University, 1956-1960
Assistant Professor, Physics Department, Stanford University, 1960-1963
Associate Professor, Stanford Linear Accelerator Center, Stanford University, 1963-1967
Professor, Stanford Linear Accelerator Center, 1967-
Technical Director, Stanford Linear Accelerator Center, 1982-

## Honors & Awards

Loeb Lecturer, Harvard University, 1974
DeShalit Lecturer, Weizmann Institute, 1975
E. O. Lawrence Medal (U.S. Department of Energy), 1976
Nobel Prize in Physics, 1976
National Academy of Sciences, 1977

## Professional Societies

American Physical Society (Fellow)
American Association for the Advancement of Science (Fellow)

## Present Committees, Consultantships & Directorships

General Motors Science Advisory Committee
Mitre Corporation - Jason Group
Visiting Committee - MIT Physics Department
Advisory Committee - Max Planck Institute, Munich, Germany
Accelerator Advisory Committee, CERN, Geneva, Switzerland
Director, Middlefield Capital Corporation
Director, Teknowledge, Inc.

1 November 1982

Mr. **Fuqua**. Thank you very much, Dr. Richter.

Let me address this to all of our panelists because I think they all proposed additions to labs they represent and could very well be affected. We try to provide these facilities to serve a rather unique community of outside users, both academic and, in some cases, industrial.

Should they be available for proprietary research by industry scientists, assuming, of course, that reimbursements costs, and so forth, are forthcoming? Does anybody wish to comment? Dr. Samios?

Dr. **Samios**. Yes. Let me speak for the National Synchrotron Light Source. We do allow proprietary research. It costs $40 an hour for full-cost recovery.

We also allow research that is not proprietary. The condition is that the information that is gathered is published in the literature within a reasonable length of time and they do not pay for the time. So there is a policy at Brookhaven which allows for proprietary research, and it is in effect.

Mr. **Fuqua**. Does anyone else wish to comment?

Dr. Shirley?

Dr. **Shirley**. I would just say that we concur with that. I think it is general policy, and I believe it is essential because these are large and expensive facilities, and they are unique. There are many instances in which industry really needs access to these facilities. So I really think that we almost have no choice.

Dr. **Zucker**. Mr. Chairman, we have a policy that not only allows it, but we also have a history of doing it. On our small-angle neutron-scattering facility, we have had three industrial organizations—Du Pont, Firestone, and Eastman Kodak—use the machine at full cost recovery. They have exposed polymers for their own proprietary use.

I might also say that in our contacts with industry, that is essential. Industry will do very little in the way of research without maintaining a proprietary position.

Dr. **Bjorken**. Mr. Chairman, to my knowledge there has never been proprietary research at Fermilab, nor is there any expectation of any in the future, although I am not——

Mr. **Fuqua**. Well, yours is a little more theoretical than some of the other work. [Laughter.]

Dr. **Bjorken**. I would comment that we do have an industrial affiliates program for technology sharing, but there is nothing proprietary, to my knowledge, about that.

Mr. **Fuqua**. Last time I was at Fermi, they were doing some medical research, using particle beams to treat cancer victims. Suppose some company were developing a spinoff of what you have at Fermi? I know it would not be that large, but they could in fact conduct some research there, could they not?

Dr. **Bjorken**. The neutron therapy facility is a medical research project which treats a large number of patients per year, including my wife, I might add, but to my knowledge, I don't know that there would be any industrial research associated with that or that there is any plan for it.

Mr. **Fuqua**. I understood that there were not any plans, but should that occur, it might be an example.

Dr. **Bjorken**. I would rather pass on that. I don't know enough.

Dr. **Richter**. Stanford has a policy on the synchrotron light source which requires publication within a certain amount of time. Industrial users may use it if their proposal will pass muster on scientific grounds, but the work must be publishable, and the delay is sufficiently long—it is months, not years—to allow time for industry to patent things which may be discovered.

I don't think we are facing a real problem right now with the use of these facilities for proprietary research. Industry is doing mostly research work now, what all of us would call research; and it seems to me that this issue is not going to become a serious point until the question of using something like a synchroton light source for production comes along.

There are a lot of people who believe that synchrotron light sources are the wave of the future in producing submicron integrated circuits. When that happens, then one will have a serious question of should Federal facilities be devoted to producing chips by, say, Intel. There I think the answer is no, and I think everybody would agree, but there is going to be a gray area in between when people go from their research to their first prototypes of a real device, and there I think it would be best for the country if we did encourage industry to use these facilities in the developmental prototype stage but to build their own if they are really going to produce 1 million integrated circuits a year with them.

Mr. **Fuqua**. Let me expand the question or maybe add a part B. How about unclassified basic research for the Defense Department?

Dr. **Samios**. Let me comment on that first, since I was first last time. We recently approved a proposal from the University of California, Los Alamos, Lawrence Berkeley, Sandia—it is a consortium—for a broad program including surface ions, X-ray scattering, and standardized sources for calibration of detectors relevant to the weapons program. This was a proposal by this consortium to the National Light Source.

Last week the program advisory committee met and approved this proposal. In this particular case, this is not classified, and it was approved and they have agreed to publish the results in the literature.

Mr. **Fuqua**. Dr. Richter?

Dr. **Richter**. Well, I guess I believe, my laboratory believes, and I think the university believes that you should not judge basic research by its ultimate end use, because nobody knows what the ultimate end use of any basic research could be.

Our university does not allow classified research on campus, and I think that is quite proper. It does not go with the air of openness that is required in an academic institution. But I do not believe there is any bar to unclassified basic research, nor do I think there should be.

Mr. **Fuqua**. Dr. Zucker?

Dr. **Zucker**. Mr. Chairman, I would like to comment on Dr. Richter's statement about proprietary position coming into play when production is involved. I believe that it comes in earlier. The facilities we are talking about here are very expensive. Industry cannot afford to build a synchrotron ring or a neutron source or a collection of high-temperature materials equipment.

To do the research necessary to develop some proprietary materials, they will have to use the national facilities, and they will have to maintain a proprietary position in the research mode before the production mode. We see that all the time.

Now, the second part of your question, as far as the Oak Ridge National Laboratory is concerned, we have no compunction about doing research that is related to national defense. We look upon ourselves as custodians of national facilities. If defense research is involved, a national facility should be properly made available.

Mr. FUQUA. Dr. Samios, if a decision were made this summer to proceed with the Colliding Beam Accelerator, what would DOE and Brookhaven, what would be your course of action, since there is no fiscal year 1984 construction funds?

Dr. SAMIOS. That is a good question.

We are having a review by DOE in April where we are presenting our plans. Our present plan is to be able to finish construction of the project by fiscal year 1988. We believe that is technically feasible. In order to do that, we would be requesting on the order of $10 million in construction money in fiscal year 1984. This is to buy material for magnets needed for producing units in fiscal 1985 since there is an appreciable lag between the time you put your order in and the time you receive the material.

So the simple answer is that in order to meet the 1988 time schedule, we would need $10 million in construction funds in fiscal year 1984.

Mr. FUQUA. In fiscal year 1984?

Dr. SAMIOS. In fiscal year 1984.

Mr. FUQUA. But DOE has not requested any.

Dr. SAMIOS. That is correct.

Mr. FUQUA. Are they prepared to request those funds?

Dr. SAMIOS. At the present time, I think the answer to that is no, until the review is finished.

Mr. FUQUA. Would they include that in a supplemental request that could come up later in the year?

Dr. SAMIOS. We would certainly urge that.

Mr. FUQUA. Dr. Shirley, there has been criticism that there has not been sufficient time for the national center for advanced materials proposal to be reviewed by the science research community, that this is a rush job. How do you respond to that?

Dr. SHIRLEY. Well, we see an important national need, and we see an opportunity to address that need. We have made efforts in a variety of contexts to make the national community aware of our plans and to seek input from the national community.

Mr. FUQUA. Could you provide us documentation of that effort?

Dr. SHIRLEY. Yes, I certainly can. I would be glad to do that.

[The information follows:]

Lawrence Berkeley Laboratory
1 Cyclotron Road Berkeley, California 94720
Office of the Director
(415) 486-5111 • FTS 451-5111

15 March 1983

The Honorable Don Fuqua
Chairman
Committee on Science and Technology
U.S. House of Representatives
B374 Rayburn House Office Building
Washington, D.C. 20515

Dear Chairman Fuqua:

Thank you for the opportunity to speak before your committee on March 3 about the National Center for Advanced Materials (NCAM). This letter is in response to your request for information regarding peer involvement in the development of NCAM. I apologize for the two-week delay, but I have just returned to Berkeley from follow-up visits (starting the day after my committee appearance) with leading scientists at Bell Laboratories, IBM, Exxon and General Electric. Support for NCAM remains very high among industrial scientists.

Following is a chronology of significant NCAM events to date. The numbers in parentheses identify the referenced documents, which are being supplied as enclosures to this summary letter.

June 1982 -- Field Task Proposal/Agreements (1-6) to the Department of Energy.

FTP/A's are the formal mechanism within DOE for proposing new programs. This document was submitted to program managers in the Basic Energy Sciences area of the Office of Energy Research. It was further refined to reflect agency and other input, and resubmitted on June 21, 1982, in total on October 22, 1982 and then again in separate packages on November 24, 1982 (the Advanced Light Source FTP/A) and on January 11, 1983 (the Scientific Programs).

July 7, 1982 -- DOE Energy Research Advisory Board, Multiprogram Laboratory Panel Visit to LBL (7).

This panel, headed by Dr. Ivan Bennett, and including industrial scientists, was provided a broad view of the Laboratory. NCAM was highlighted in my discussions of "The Role of the Lawrence Berkeley Laboratory."

July 26, 1982 — Federal Laboratory Review Panel of the White House Scientific Council visit to LBL (8-9).

This panel, headed by David Packard of the Hewlett-Packard Company and including other industry scientists and a representative of the National Academy of Sciences, was briefed on the current plans for NCAM Science and the Advanced Light Source. (See, especially, the answer to Question L (document 8, p. 7), "Can you perform research related to material needs other than those you are currently involved in?")

September 2, 1982 -- Institutional Planning Review (10-11).

NCAM was given prominent treatment in this briefing of DOE management responsible for the institutional planning of the Laboratory.

November 15-16, 1982 — NCAM Construction Project Review (12).

In keeping with DOE policy for Major Energy Acquisition Projects, NCAM was reviewed by a team headed by Dr. Edward Temple, Office of Management, to assure that the project was ready for construction, both technically and in terms of project management. Also in November, the Preliminary Design Handbook for the Advanced Light Source (13) was printed for distribution to interested scientists.

January 6, 1983 — Advanced Light Source Technical Review (14).

Called at my request, a committee of the world's leading experts in the design, construction and operation of synchrotron light facilities reviewed our plans for the ALS. Their report, in its entirety, is provided as document 15.

January 10, 1983 — NCAM Scientific Review (16).

Details of program plans for the FY 1984 startup were presented to the DOE managers and guest scientists shown on the attendee list. Included were many members of the Council on Chemical Research, which represents some 40 individual and academic organizations, as well as the Chairman, Past Chairman, and Chairman-elect of the Solid State Sciences Committee of the National Academy of Sciences.

February 8, 1983 — Forum of the Solid States Sciences Committee.

It was my pleasure to share the program with Chairman Fuqua. In the 25 minutes allocated me I gave an overview of the project.

I have personally discussed NCAM with chief executive officers, vice-presidents for research and working scientists in American industry and universities. I have also been assisted in this effort by other LBL scientists. A partial listing of organizations contacted is given below, and correspondence (17), where available, is appended. U.S. Steel Corp., Exxon Research and Engineering Company, Bell Laboratories, Mobil Research and Development Corp., Air Products and Chemicals, Inc., Catalytica Associates, Inc., The Dow Chemical Company, Union Carbide Corp., Intel Corp., Hewlett-Packard, Xerox, General Atomic, Westinghouse, Atlantic Richfield Company, Council for Chemical Research, The Boeing Company, Chevron Research Company, IBM Corp., Varian Associates, GTE Laboratories, Inc., The Procter and Gamble Company, 3M.

Our plans for the months ahead include another construction readiness review in April and a workshop to discuss in detail the kinds of science to be done at the ALS (the ALS Users Workshop, tentatively scheduled for May 23-27). We also look forward to discussing NCAM with the ad-hoc review committee being put together by Dr. Trivelpiece.

Mr. Chairman, I submit that the foregoing chronology demonstrates that NCAM has been and continues to be extensively presented, studied, discussed and commented on by a large number of peers, both in formal sessions as well as informally and individually. The general reaction has been strongly favorable to our proposal.

I would further suggest, Mr. Chairman, that there are other substantially reliable measuring sticks that may be used to assess the ability of a Laboratory to undertake a major new program with such important national implications as NCAM offers. For example, I think you will find interesting the enclosed information (18) extracted from the list of "most-cited scientists" compiled by the Institute of Scientific Information. You and your committee colleagues (and your predecessors) can take justified pride in the fact that the National Laboratories show up so well, inasmuch as this is a reflection of the strong support Congress has given to scientific research over the years. At the same time, I hope it is pardonable pride for me to point out the contribution made to this list by LBL (14 listed) and SLAC (9). Other measures include the number of members elected to the National Academies of Science and Engineering (32 scientists and engineers) and the premier standing of the University of California at Berkeley graduate schools in the Physical Sciences (document 19). Believe me, with this quality of peer pressure, we are ever mindful to be on our toes.

Sincerely,

David A. Shirley

David A. Shirley
Director

Enclosures

Dr. **Shirley**. We are going to have to continue the review process. In fact, we have solicited input from a variety of industrial scientists and industrial research leaders, and we have a number of supporting letters which do give us indications of support for NCAM and desire to participate in NCAM.

The peer review process for materials science, especially as broadly construed as materials science is in the NCAM proposal, is not as well established as it is, for example, in high-energy physics, where there exists a high-energy physics advisory panel and a pretty clearly delineated set of steps for peer review.

We believe strongly in peer review. We exercise it voluntarily. It is a very important component of our review processes to insure the quality of research in our laboratory. In fact, the last workday before leaving Berkeley, I participated in a review by an outside committee of our materials research effort, which we conduct annually. The committee consisted of representatives, four from industry and two from laboratories, and they did an indepth review of our program involving visitations of laboratories, discussions with principal investigators and their students, and they gave a very detailed critique of the performance and quality of our program, to which we will be responsive.

Mr. **Fuqua**. Would you find it acceptable to delay the construction of NCAM while a scientific peer review panel was appointed by DOE to come up with a study or a review?

Dr. **Shirley**. I think it would be inadvisable to delay the initiation of construction beyond fiscal year 1984. DOE is in fact appointing an advisory board to oversee the formulation of the scientific program.

Mr. **Fuqua**. But is not that something like after the fact? The point I am trying to make is, there are those who say that that is the reason it should be delayed, that it has not been thoroughly reviewed and that we are saying we are going to do it and then we are going to review what we are going to do.

Dr. **Shirley**. We have already had some review by the Department of Energy and the various review panels that have visited the laboratory, and we are continuing to interact with the scientific community on the NCAM program

Mr. **Fuqua**. Dr. Zucker, how would the proposed lab at Berkeley impact on the high temperature materials lab at Oak Ridge?

Dr. **Zucker**. Well, there are probably two aspects of that. One is the technical aspect. I don't believe there is a great deal of overlap—I would say there is no overlap—between the two installations.

We, as you saw, are interested mostly in materials that are used for their structural and mechanical and physical properties. We are interested in areas of energy production, energy conservation, and, in general, from a basic point of view, trying to understand the mysteries of alloys and ceramics, which are very little understood and which come into play at these high temperatures. I don't believe that NCAM is going to play a large role in that field, or any role at all.

On the other hand, there is the question of funding, and there I think it is a matter of the Congress deciding what to fund and what not to fund and where the money goes: If it goes in one way, is there enough money left for another laboratory? So I suspect there may be some considerations of this sort that are in your mind, and they are also in ours, of course.

Mr. **Fuqua**. Why is a new building needed at Oak Ridge? Why can't you use your present or newly acquired equipment in some existing buildings that you have there?

Dr. **Zucker**. First of all, there are no existing buildings that have the right kinds of properties where you could install the equipment. We would have to refurbish existing buildings, and that turns out to be a very expensive operation.

Mr. **Fuqua**. Is it more expensive than a new building?

Dr. **Zucker**. To do the whole thing, to do the equivalent thing inside an existing building is more expensive than to build a new building, yes, sir. It costs more to take a room that already exists, rip out all the stuff and put it all back in, than to pour a pad and build a new building. That is one of the mysteries of building that I have come to believe, but it has taken me about 5 years to believe that. It is true.

Mr. **Fuqua**. Maybe we had better expend some effort in better understanding that. [Laughter.]

Dr. **Zucker**. We have competitive bids on all our construction. I always used to tell my engineers that they didn't know what they were doing when they estimated it, but I am now a believer. I don't understand it from fundamental principles, but I am a believer.

But there are other things that are involved here, I think, besides building costs. We would like to have a building that would provide equipment for all the people in that building to use. There will be certain specialized things like acoustic microscopes and things of that nature that will be used by the various groups.

We also feel that the interaction between the groups is essential. Each group will be doing both basic and applied research, and each group's effort will depend on other group's cooperation.

We also need space and facilities for users. We envision 30 to 50 users in residence in any 1 year, and these people have to sit somewhere, they have to have a library, they have to have seminar space and things of that sort. So we really feel that we need a center for high-temperature materials research in which we can organize all these groups, in which we put the new equipment, which will then be a technical center in the country for high-temperature materials science.

Mr. **Fuqua**. Can you provide us with similar information to that I asked of Dr. Shirley on the documentation of the support from the scientific community of the facility at Oak Ridge?

Dr. **Zucker**. Yes, sir.

Mr. **Fuqua**. When I say facility, I mean the lab, not the building.

Dr. **Zucker**. Yes, sir, I can.

[The information follows:]

0321-17-83

OAK RIDGE NATIONAL LABORATORY
OPERATED BY
UNION CARBIDE CORPORATION
NUCLEAR DIVISION

POST OFFICE BOX X
OAK RIDGE, TENNESSEE 37830

March 21, 1983

Mr. Ezra D. Heitowit
Committee on Science and
Technology
Room B374
Rayburn House Office Building
Washington, D.C. 20515

Dear Mr. Heitowit:

Subject: High Temperature Materials Laboratory Supporting Information

Dr. Alex Zucker has asked me to provide supporting information pertaining to the High Temperature Materials Laboratory (HTML) being considered for construction at the Oak Ridge National Laboratory. This information is being supplied as a result of the request of Mr. Fuqua to Dr. Zucker of ORNL during his testimony on March 3, 1983. From the testimony transcript, that request was "Provide the evidence that the proposed HTML has been reviewed and is supported by the materials research community."

There are two kinds of information which support the justification and need for the HTML. The first is generic in nature and was generated within the recommendations of several of the nine national workshops organized by the Materials Sciences Program staff of the Division of Basic Energy Sciences, Energy Research and Development Administration (ERDA) during the period March through May, 1977. The second kind of support has resulted from overview and assessment committees which have been constituted either to judge the quality and direction of research divisions at ORNL that will have a substantial role in the HTML, to judge the quality and direction of ORNL, or to address the question of the appropriateness of the HTML and its geographic location.

The attachments include (1) the more generic supporting material from the conclusions of the ERDA workshops, (2) a chronological summary of the relevant statements of the advisory committees, and (3) results of discussions about the HTML concept with the managers of applied energy technology programs in the Department of Energy. Full copies of items (1) and (2) are available from Dr. Zucker, if the committee requires them.

Sincerely,

V. J. Tennery

V. J. Tennery, Director
High Temperature Materials Laboratory

## GENERIC SUPPORTING MATERIAL for HIGH TEMPERATURE MATERIALS RESEARCH

In the spring of 1977 a series of nine workshops was held to ascertain new directions and priorities for energy related research in the Materials Sciences Program. A summary report, edited by W. L. Clinton,[1] J. R. Cost,[2] and L. C. Ianniello, was published in August 1977 and is identified as ERDA 77-76/2, *Materials Sciences Overview, II. Workshop Reports*. The Foreword was written by Donald K. Stevens, Assistant Director, Materials Sciences Program, Division of Basic Energy Sciences, ERDA.

Information concerning the organization and makeup of these nine workshops is given here.

| Workshop Title | Location | Organizing Group |
|---|---|---|
| 1. Electronic, Magnetic, Optical and Thermal Properties[1] | ANL | D. Price (ANL)<br>A. Freeman (Northwestern U.)<br>M. Cohen (U. of Chicago)<br>P. Seiden (IBM) |
| 2. Surface Science[1] | LBL | G. Somorjai (LBL)<br>J. Yates (NBS) |
| 3. X-Ray, Neutron and Electron Scattering[1] | ORNL | M. Wilkinson (ORNL)<br>A. Bienenstock (Stanford U.)<br>J. Rush/J. Rowe (NBS)<br>M. Blume (BNL) |
| 4. Low Temperature Research and Superconductivity[1] | BNL | M. Strongin (BNL)<br>J. Mercereau (Cal Tech)<br>J. Wheatley (UC/San Diego) |
| 5. Phase Transformations, Stability and Materials Interactions[2] | LBL | W. Morris (LBL)<br>L. Brewer (LBL)<br>P. Shewmon (Ohio State U.) |
| 6. Defects, Diffusion and Radiation Effects[2] | ORNL | F. Young (ORNL)<br>J. Stiegler (ORNL)<br>A. Nowick (Columbia U.) |
| 7. Engineering Materials Science[2] | ORNL | C. McHargue (ORNL)<br>C. Wert (U. of Illinois) |
| 8. Mechanical Properties[2] | ANL | F. Kocks (ANL)<br>A. Evans (Rockwell Int.) |
| 9. Thermodynamics and Electrochemistry[2] | ANL | F. Cafasso (ANL)<br>N. Nachtrieb (U. of Chicago)<br>L. Darken (Penn State U.) |

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Bill Clinton (1) and Jim Cost (2) were on leave to the Materials Sciences Program from Georgetown University and Purdue University, respectively, and were members of the designated workshop Organizing Group.

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The attendees at this series of workshops included a broad spectrum of the national materials community. The composition of these attendees for each of the nine workshops is shown in the following table.

| Workshop Title (Total Attendees) | | No. of attendees representing | | | | |
|---|---|---|---|---|---|---|
| | | ERDA | Industry | National Lab | NBS/Other Government | Academic |
| 1. Electronic, Magnetic, Optical and Thermal Properties | (51) | 3 | 15 | 11 | 0 | 22 |
| 2. Surface Science | (51) | 3 | 9 | 20 | 4 | 15 |
| 3. X-Ray, Neutron and Electron Scattering | (49) | 2 | 6 | 24 | 6 | 11 |
| 4. Low Temperature Research and Superconductivity | (41) | 4 | 3 | 13 | 3 | 18 |
| 5. Phase Transformations, Stability and Materials Interactions | (46) | 2 | 14 | 15 | 2 | 13 |
| 6. Defects, Diffusion and Radiation Effects | (42) | 4 | 7 | 15 | 0 | 16 |
| 7. Engineering Materials Science | (43) | 5 | 7 | 17 | 2 | 12 |
| 8. Mechanical Properties | (22) | 3 | 1 | 9 | 0 | 9 |
| 9. Thermodynamics and Electrochemistry | (38) | 4 | 7 | 11 | 1 | 15 |

Generic comments in support of the need for high-temperature materials research have been extracted from the Conclusions of the Workshops and are noted on page 3 of this attachment.

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## GENERIC SUPPORTING MATERIAL for HIGH TEMPERATURE MATERIALS RESEARCH
(extracted from the Conclusions of ERDA Workshops)

1. Need for studies of the effect of aggressive environments on the mechanical properties of alloys (recommended in six of the nine workshops).

2. Point defects largely control atomic transport in metals and alloys, but "good information ..." concerning the properties of lattice defects" "... is largely missing for bcc and hcp metals important to higher temperature applications in future energy systems." (Workshop on Defects, Diffusion, and Radiation Effects, p. 6-15*).

3. Basic research on the kinetics and mechanisms of corrosion, erosion, and scaling phenomena under extreme enviromental conditions" deserves a priority of the first rank. (Workshop on Thermodynamics and Electrochemistry, p. 9-1).

4. "There is particular value in extending the range of experimental investigations of phase equilibria to higher temperatures such as may be encountered in ... high temperature energy conversion schemes." (Workshop on Thermodynamics and Electrochemistry, p. 9-10).

5. "In particular, creative new techniques for high temperature measurements of thermal expansion, conduction ... and heat capacity are needed ..." (Workshop on Electronic, Optical, Magnetic, and Thermal Properties, p. 1-7).

6. "Many ceramic materials for high temperature applications are nonstoichiometric, or become so under severe conditions of operation. Investigations of the conditions that cause dissociation and phase transitions of ceramic materials are needed. Techniques for measuring and working at temperatures in excess of 2000°C in both oxidizing and reducing atmospheres must be developed." (Workshop on Defects, Diffusion, and Radiation Effects, p. 6-23).

7. Evolving Microstructures. "A major shortcoming of (the classical approach of physical metallurgy) has been slow progress in the area where microstructure changes and mechanical deformation happen simultaneously." "... the effect of ongoing transformations during plastic flow and fracture has received little attention. Similarly, the phenomenon of dynamic recrystallization and associated twinning is only beginning to be understood mechanistically. High-temperature energy technologies require a new emphasis on research in these critical areas." (Workshop on Mechanical Properties, p. 8-6).

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*Page references are to *Materials Sciences Overview II. Workshop Reports*, W. L. Clinton, J. R. Cost, and L. C. Ianniello, eds., ERDA 77-76/2.

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## ADVISORY COMMITTEE RECOMMENDATIONS for a HIGH TEMPERATURE MATERIALS LABORATORY at OAK RIDGE, TENNESSEE

(paraphrases from Annual Reports)

### 1. HTML Advisory Committee

This Committee was formed in early 1978 for the purpose of evaluating the HTML concept and providing recommendations to ORNL on the content of a high-temperature materials research program. As a result of meetings at ORNL of this Committee, many useful recommendations have been made as to how to best organize and conduct the high-temperature research program and to make the special research equipment in the building available to researchers from the university and industrial communities.

In February 1978 this Committee enthusiastically endorsed the concept for the HTML and, in November 1979, recommended that a high-temperature materials research program be initiated at ORNL as soon as possible. This program was started at ORNL shortly thereafter within the DOE Materials Sciences program and, in September 1981, a permanent Director for the HTML was appointed.

The members of this Advisory Committee and their affiliations are listed here.

**Dr. Louis F. Coffin**, Metallurgy Laboratory, General Electric Research and Development Center
**Dr. F. S. Pettit**, Department of Metallurgical and Materials Engineering, University of Pittsburgh
**Dr. J. R. Rice**, Division of Engineering, Brown University
**Dr. Gerd Rosenblatt**, Department of Chemistry, Pennsylvania State University (Now at Los Alamos National Laboratory)
**Dr. Marion Semchyshen**, Climax Molybdenum Company of Michigan
**Dr. D. Stein**, Department of Materials Science, Michigan Technological University
**Dr. J. B. Wagner, Jr.** (Chairman), Center for Solid State Science Arizona State University
**Dr. W. W. Williams**, Department of Physics and Ceramic Engineering, University of Illinois

### 2. HTML Industrial Users Committee

This Committee was formed in late 1980 for the purpose of informing the U.S. industrial community of the HTML concept and for providing advice and guidance on mechanisms and organization for the high-temperature materials research program which would be supportive and/or related to interests and requirements of U.S. industrial high-temperature materials. The Committee met at ORNL in December 1980 and made several comments and recommendations, including:

(a) expressed strong support for the HTML concept,

(b) noted the important role that the HTML could play in assisting the U.S. industrial materials community,

(c) recommended that a procedure be identified whereby industrial researchers could come to ORNL and work for various periods of time once the HTML is constructed,

(d) recommended that a senior industrial researcher be identified to work with the Director once the HTML is constructed

(e) recommended that a careful analysis of the patent situation be conducted to minimize potential conflict in jointly funded (government and industry) programs and for assigned or loaned industrial employees,

(f) decided that a national meeting be held at ORNL for senior industrial research management to introduce them to the HTML concept and for making them aware of some of the on-going high-temperature materials research at ORNL.

As a result of item (f), a meeting, entitled *Materials Research Highlights*, was organized and held with the assistance of this Committee at ORNL on April 23–24, 1981. Twenty-six representatives (holding positions of the Vice President or Director of Research) of U.S. firms attended this meeting, were told of the HTML concept, and heard presentations on selected ORNL research. The response was so enthusiastic that several contacts have subsequently occurred from these companies with the objective of learning how they and ORNL could conduct research together. Some of these contacts have resulted in collaborative activities, for example, INCO, Bendix, Babcock & Wilcox, and Cabot Corporation. At the end of this meeting, the Committee concluded that the industrial community was supportive of the HTML and that its construction would be a unique opportunity to foster government laboratory/industrial research in high-temperature materials.

The members of this Industrial Users Committee and their affiliations are as follows:

Dr. Elihu F. Bradley, Chief Materials Engineer, United Technologies
Dr. Arthur W. Dana, Jr., Vice President, Fansteel Corporation
Dr. R. F. Decker, Vice President, INCO, Limited
Dr. E. H. Kottcamp, Jr., Manager, Product Research, Bethlehem Steel Corporation
Mr. W. D. Manly (Chairman), Senior Vice President, Cabot Corporation

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## 3. Research Division Advisory Committees

One mechanism found to be extremely useful for assessing and maintaining strong technical programs in the research divisions at ORNL utilizes Advisory Committees comprised of outside experts in their fields of materials. Members of these committees visit researchers in the respective divisions during the year and then meet together at ORNL and submit a report on their respective division to the Laboratory Director. The committees for the Metals and Ceramics Division have repeatedly encouraged the concept of the HTML and a strong program in high-temperature materials research. Extracted excerpts from these committee reports follow. The Committee Chairman is given for each excerpt.

### Metals and Ceramics Division

1981: *The Committee recommends that strong efforts be continued in promoting the HTML project.*

Arden L. Bement, Jr., TRW Corporation

1980: *The Committee recommends that ORNL consider procedures and policies to insure that industrial and academic personnel can make maximum utilization of the HTML when this important project is operational.*

*ORNL should proceed with the definition of the operating and funding plan for the High Temperature Materials Laboratory.*

Manville J. Mayfield, TVA

1979: *The uncertainties regarding the scope, participation, and funding of the HTML is having an adverse effect on the morale of those who anticipate being involved. It is recommended that a favorable decision regarding funding of this facility be made as soon as possible.*

W. R. Hibbard, Jr., Virginia Polytechnic and State University

1978: *The plans underway or being considered for the future to establish unique high technology equipment centers, such as the High Temperature Materials Laboratory, are good.*

P. M. Brister, Babcock and Wilcox

1977: *The Committee understands that both the concept of an HTML and its location at ORNL have been accepted by ERDA (DOE) and that budgeting for it is in progress. Because of its importance and the interval that will be required to build and implement it after funds become available, it is hoped that funding in FY 78 will be achieved.*

H. Palmour III, North Carolina State University

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## ORNL Management Advisory Council

This Advisory Committee to the ORNL management for the years 1977 through 1981 was composed of the members listed below. The chairmanship of the committee rotated to a different member each year.

Dr. W. O. Baker, President, Bell Laboratories
Professor Manson Benedict, Nuclear Engineering, M.I.T.
Professor D. Allan Bromley, Chairman, Department of Physics, Yale U.
Dr. W. D. McElroy, Chancellor, University of California, San Diego
Mr. Eric Reichl, President, Conoco
Mr. J. C. Rengel, Executive Vice President, Nuclear Energy Systems, Westinghouse
Dr. J. A. Swartout, Consultant

This Advisory Councils' concern and interest in a high-temperature materials laboratory are reflected here.

1981: *We are pleased that ORNL is aggressively seeking to acquire the necessary equipment for a High Temperature Materials Laboratory and that this Laboratory will bring a large number of industrial users into contact with ORNL basic research activities.*

1980: *We foresee substantial increase in ORNL-University interaction in establishing a strong multidisciplinary program that will eventually utilise the facilities of the planned High Temperature Materials Laboratory.*

1979: *The Committee is enthusiastic about the potential of the High Temperature Materials Laboratory both for work within a number of the programs at Oak Ridge and as the focus of national activity in this increasingly important area of science. We believe that excellent progress has been made toward insuring that the Laboratory will be an effective addition to the national program and that its structure will serve both the academic and industrial communities well.*

1977: *We congratulate ORNL management on its initiative in proposing to construct a national high-temperature materials laboratory facility. This is clearly needed and, equally clearly, fits extremely well, as proposed, into the ORNL framework. We are impressed by the planning for the Laboratory and its research program. We suggest that the new Laboratory, from its inception, move aggesively to function as the central information repository and exchange for high-temperature materials behaviour and phenomena. There is a marked national lack currently in this area that ORNL, building upon its already extensive information activities, could fill in unique fashion.*

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## SUPPORT FOR THE HTML FROM MANAGERS OF APPLIED TECHNOLOGY PROGRAMS, DEPARTMENT OF ENERGY

The HTML is intended to include both basic and applied research on high-temperature materials with the annual operating budget being eventually consituted of approximately equal proportions of these two types of projects. With this concept in mind, the Director and the Associate Director of Physical Sciences of the Oak Ridge National Laboratory held verbal discussions with the managers of the fossil, fusion, conservation, and fission programs in the Department of Energy during 1980 and received, in all cases, an endorsement of the need for and appropriateness of the HTML concept from the managers of these technology programs.

Dr. **Zucker**. We have had numerous committees; we have had user groups; we have had study groups, and we have reports, and I will be sending that to you.

Mr. **Fuqua**. Thank you, sir.

The Tevatron facility at Fermi, is there going to be any further cost escalation on that project?

Dr. **Bjorken**. Which project are you referring to? Saver or Tevatron II?

Mr. **Fuqua**. Tevatron I.

Dr. **Bjorken**. Tevatron I? Not to my knowledge, no.

Mr. **Fuqua**. You think that is under control, then?

Dr. **Bjorken**. I believe that is correct, yes.

Mr. **Fuqua**. In the high energy physics community, in planning for the next generation of accelerators, do you see the possibility of discussions with the Western Europeans, particularly the group that is at CERN, about a joint project or some method of international cooperation in the fields? These are very, very expensive machines.

We have international teams. We invite the international community to use our equipment. It seems to me we are going to have to explore possibilities of more cooperation so we can share some of the costs of these machines. Is that being explored? Is that feasible?

Dr. **Bjorken**. Well, there is now an increased sharing of utilization with machines built by given regions. We see in this country increased participation of U.S. scientists in the DESY Laboratory in Germany and in the CERN program as well. Likewise, because the fixed target program at Fermi Lab, the Tevatron II, is more than twice the energy of the corresponding fixed target program in Europe, we see an increase of interest of European scientists to do experiments at Fermilab.

And so there is a considerable amount of interchange at that level on the mounting of large experiments in other regions, or even the moving of apparatus from one region to another.

When it comes to new facilities, there does exist some mechanism, international mechanism, for discussion of an international program. This is the committee called ICFA, International Committee for Future Accelerators, which was originally formed to consider, among other things, the possible ultimate construction of a machine which was too large and too costly for any given region to build by itself and which may be the inevitable course in our field in the longer run.

In a projection of, I would say, the next 10 years, one does not see that happening. However, the ICFA organization, among its other charges, does have the opportunity to foster a cooperative atmosphere between the different regions in terms of international planning for future facilities.

I agree with you. I think we should make every effort to have a program that, on the international level, makes sense and does not duplicate facilities.

Mr. **Fuqua**. Dr. Richter, would you like to comment?

Dr. **Richter**. May 1? I, in fact, am one of the three U.S. delegates to this funny organization called ICFA. Dr. Bjorken is one, also. That has representatives from all over the world—the Russians, the Europeans, the Americans, the Japanese—and it has been talking for a few years about world cooperation on a big accelerator.

One of the problems is that the political difficulties of arranging such a collaboration are enormous. Until the accelerators become very expensive, facing the political problems of choosing a site among the Americans, Japanese, and Russians is overwhelming.

The ICFA organization, until now, has been spending most of its time, I think very profitably, arranging workshops and conferences on accelerator technology: How can we bring down the costs of accelerators? What are the limits to their growth?

It is beginning to grapple with these other social questions, but they are really very tough, and it is going to be quite a while before we see something worked out.

Mr. FUQUA. Dr. Richter, I asked the other day in a hearing about the timeframe for the collider at Stanford. Why does it have to be built in 3 years? Why can't we build this in 4 or 5 years?

Dr. RICHTER. You could build it in 4 or 5 years. I think every project has its own natural time scale. A natural time scale for this one is 3 years. If you stretch it out, when you stretch out any projects, staff costs go up, escalation goes up, and to stretch it out will cost money.

There is a further thing, and that is clearly that there is a competitive sense between U.S. physicists and European physicists.

Mr. FUQUA. That was my next question. The discovery of the W-particle, does that have any impact on the schedule?

Dr. RICHTER. No, because we have been requesting this thing on a 3-year schedule since before they discovered the W-particle. But the Europeans are building this electron positron machine LEP. It is now scheduled to turn on sometime around the beginning of 1988. There is a natural feeling that we would like to get a look first. It is nicer to make discoveries yourself than to read about them in the journal. Although everybody is rather pious at times, talking about how it is all for the best of science, you would really like to do it yourself rather than reading about it, and I ought to be honest about it.

But, economically, it is a fact that stretching the project out will increase its budget.

Mr. FUQUA. Dr. Samios, are you prepared for Mr. Carney? [Laughter.]

Dr. SAMIOS. Our ethnic backgrounds are so similar. [Laughter.]

Mr. FUQUA. Mr. Carney.

Mr. CARNEY. Look, I'm an AHEPA'n. I hope you know that. [Laughter.]

Mr. FUQUA. An AHEPA'n?

Mr. CARNEY. That is an organization of Greek Americans of which I am a member. It was started in Atlanta, Ga., down in the South part of our country.

Anyhow, I don't want to talk to Nick; I see him enough. I would like to talk to Dr. Shirley if I can.

Dr. Shirley, on your new program out there, I was curious to know why you have made a budget request for $38 million and you are now just going out to the community of users. Is that the cart before the horse?

Dr. Shirley. Well, we first proposed this NCAM initiative last year to the Department of Energy and later to the White House Science Panel Federal Laboratory Committee who visited our laboratory in July. We have presented it at divisional reviews within the laboratory, and we have made the Solid State Sciences Committee's Panel on Synchrotron Radiation aware of our proposed light sources as early as last summer when they were meeting to review the need for synchrotron radiation.

The community is really a very broad community. There are chemists, there are physicists, there are materials scientists, and so forth. I think that we have covered some reasonable fraction of the community. We obviously have not reached all of them.

Mr. Carney. It seems to me, though, that we are moving forward rather quickly here. You have not really gone out to the user community. There has not been much peer review except for in-house. You made a proposal, a decision was made, and I am not so sure if we might be going too fast.

I, in no way, shape, or form, have any objection to this type of endeavor. I would strongly support it. I just hope we are not running out there too fast instead of being a little bit more conservative and making sure that we are touching all the bases and that everyone is on board and the project will be a success.

There seems to have been very little review. In your testimony, you mention that you are going out to the four major labs on the east coast to talk to what I would consider—I am not sure if I am right or wrong—users to get input, but yet we are moving ahead before we have taken those steps.

I would hope that we are not going to find ourselves in a position where we might have made some mistakes. If we are a little more prudent and a little more careful, we might eliminate mistakes. Perhaps mistakes will not happen, I don't know, but the situation looks different when we get into these large expenditures.

Dr. Shirley. Should I respond?

Mr. Carney. I would love you to.

Dr. Shirley. All right.

I am personally committed to insure, through extensive review process as we go forward to implement the scientific program, that every element of the scientific program is thoroughly peer reviewed.

We are holding a series of workshops this spring. We have already held workshops on the machine parameters. We have scheduled a workshop as well on the electronic materials component of the NCAM proposal. We continue to formulate plans for further workshops.

We have interacted with industries in the bay area last year. We have had visits from Xerox. We have had visits to Hewlett-Packard. We have discussed with the laboratories I will be visiting and with others the plans that we would like to formulate. We are very receptive to continued input from throughout the user community.

Mr. Carney. The advanced light source that you will be using, is that state-of-the-art? Are you going to be building a machine as you go along, that type of approach?

Mr. Shirley. I would say that it is ahead of the state-of-the-art in the sense that it will be the first of a new generation of machines. It will be explicitly designed to utilize long, straight sections and very low emittence in the electron beam, which will give a brightness that will exceed the present or proposed brightness of other machines by two orders of magnitude.

It will also have a very short pulse structure to allow investigation of phenomena on a scale of picoseconds,—1 picosecond is 10–12 seconds, or a millionth of a millionth of a second.

Mr. CARNEY. Let me go back again on this. I am saying, can you go to the shelf and pull out plans and build this thing, or are you going to be designing and engineering this thing as you move along?

Dr. SHIRLEY. I believe we have the technology in hand to build it with our existing accelerator expertise and with the development, particularly, of the permanent magnet insertion devices which LBL has played a pioneering role in developing. We built the first insertion device back in 1979-80 based on permanent magnets and installed it at the Stanford Synchrotron Radiation Laboratory. So I feel that we are well along on the technical path toward constructing this machine.

Mr. CARNEY. Thank you.

My personal observation, for whatever it might be worth, is that I think we are going a little faster than normal in that area, but that is just a personal observation.

Maybe I will go to Nick for a minute.

Dr. Samios, you mentioned to the chairman $10 million as a sum of money you thought would be appropriate if you got a positive report from Woods Hole, which I assume will be coming out in the next couple of months. That $10 million would be for what?

Dr. SAMIOS. The magnet material.

Mr. CARNEY. It would be basically buying the materials to keep you ahead of yourself?

Dr. SAMIOS. Right, because there is a lag between the time you place the order and the time you get it, and so if we don't place it in 1984, it will not allow, in our present schedule, a 1988 turn-on.

Mr. CARNEY. I am not so sure how the jobs bill is going to be finally approved and what that might mean to all the laboratories. There might be available in that program monies for all our national laboratories. I think it would be monies well spent. It would have a double thrust. But if the purchasing of materials were eliminated from jobs bill money, is there any other type of work that you might be able to do through that program, since you don't have construction money in fiscal year 1984?

Dr. SAMIOS. I would say the area in which that would be applicable is in the remaining conventional construction that involves two areas which, with this pause, we did not complete. This requires a sum on the order of $10 million.

Mr. CARNEY. One of the prerequisites of the jobs bill—and I will ask this of everyone; I will ask Dr. Samios first—is that proposals being made should be ready to go. We are trying to do this quickly, stimulate the jobs market as quickly as possible.

Dr. SAMIOS. That was my response.

Mr. CARNEY. Can you do that? If you said, Hey, boom, we are going to give every laboratory, here's $10 million. Are you ready to do some types of brick-and-mortar things right now?

**Dr. SAMIOS.** Yes.

**Mr. CARNEY.** Dr. Shirley, would you be in that same position on your program, in the advanced materials program? Do you think, if we were fortunate to get money through that area, the jobs bill, would you be in that position?

**Dr. SHIRLEY.** Yes, sir, we would. We would be ready to spend $6 million to $8 million as early as next month on the surface science and catalysis laboratory and components of the advanced light source.

**Mr. CARNEY.** Dr. Richter, on your project, are you in a position, if we were fortunate to get money in that area, to do that?

**Dr. RICHTER.** I think we could have the first bulldozer on site in July, and we could get about $15 million of conventional construction under contract in this fiscal year and have the contractor started.

**Mr. CARNEY.** Very good. Dr. Bureaucrat? [Laughter.]

**Dr. BJORKEN.** I wish I were when I am asked a question like this.

In the Tevatron II program, there could be an acceleration of the schedule there involving a large fraction of the fiscal year 1985 money a year earlier. In other words, we could speed up Tev II very easily.

The Tev I, there is a lot of brick and mortar already, and that is under study. I cannot answer directly on that one.

**Mr. CARNEY.** Dr. Zucker, that building that would be cheaper to build from scratch, would you be in a position to quickly put that on line if we were fortunate enough to get the money?

**Dr. ZUCKER.** Yes, we are ready, but I believe the bidding process is something that we all have to undergo.

**Mr. CARNEY.** Right. I fully appreciate that.

**Dr. ZUCKER.** Some of the optimism at this table has to be tempered by the real bureaucrats that we all face.

We are ready, of course. We have title I design.

I might say, in response to your question, that we have experience in that. We have refurbished buildings, say, in our fusion laboratory, and we know what it costs to refurbish existing buildings. Our engineers actually make sense when they say that. If you have to tear something out, that costs you money. It is harder to put air conditioning and electrical ducts and all of that into an existing building. So it is paradoxical but true.

**Mr. CARNEY.** I am not a scientist, Dr. Zucker, but I can fully accept its being cheaper to start from the ground up.

**Dr. ZUCKER.** Well, I am glad you do. Anyway, we are ready.

**Mr. CARNEY.** Maybe that is a simple minded concept that I can grasp readily.

**Dr. ZUCKER.** We are ready. I mean, to answer your principal question, we are ready.

**Mr. CARNEY.** Good. So in all five laboratories, then, we could take advantage of these programs.

Now, if I may be parochial for just a couple more minutes, Mr. Chairman——

**Mr. MINETA** [as acting chairman]. Please.

**Mr. CARNEY.** Dr. Samios, would you have any objections to taking one of those halls and dedicating it to the scientists from the University of Florida and Florida State University?

Dr. **Samios**. With pleasure. [Laughter.]

Mr. **Carney**. I have no idea why that thought came to my mind, but I think Martha Krebs has some idea. [Laughter.]

At that point, I will yield back the balance of my time, Mr. Chairman.

Mr. **Mineta**. Thank you very much, Mr. Carney.

We have a call in the House right now for a vote on the rule as it relates to the jobs bill that Mr. Carney was referring to. So at this point I think we will recess for 10 minutes and then come back to start the questioning again.

Thank you very much.

[Short recess taken.]

Mr. **Mineta**. The subcommittee will reconvene. We apologize for this delay.

With this very eminent panel that we have before us, I see in the audience another very eminent scientist who has been connected with not only the scientific community but, more specifically, with the University of California. Not again being parochial, Mr. Carney, but being a proud Berkeley graduate, I am really pleased to introduce Dr. Glenn Seaborg, Nobel laureate. It is great to have you here with us.

Dr. **Seaborg**. Thank you very much.

Mr. **Mineta**. Dr. Shirley, you seem to be attracting a great deal of attention during these hearings, during this whole series of hearings on the budget for the Department of Energy.

A number of questions have come up about the National Center for Advanced Materials, and I would like to ask one. The Federal Government will be spending about $1 billion in materials sciences just this year alone, through about 14, as I recall, different agencies.

I am wondering, how does your suggestion of a national center for advanced materials fit into this effort?

Dr. **Shirley**. Mr. Chairman, I believe that the National Center for Advanced Materials will bring a focused effort to bear on a long-neglected area of materials science in the Department of Energy program. I think this will impact on the whole DOE materials science program because it is advanced materials explicitly. We are looking at the problems that will arise tomorrow in materials science programs, in the 5- to 15-year timeframe which is just at or over the horizon of many of our high-technology industries.

I believe that there is a need for this focused effort in the areas that we are advocating programs in; specifically, surface science and catalysis, where we are just learning to understand surfaces on an atomic scale. We need to concentrate advanced instrumentation on these problems, in the synthesis of advanced materials and in advanced device concepts.

We are also, Mr. Chairman, doing an experiment here in relationships of universities and industry and a major laboratory which I hope will serve as a prototype for collaboration of this kind in other areas of materials science.

Mr. **Mineta**. We heard testimony yesterday about the need for industrial participation in this effort at the earliest possible stage to insure that the facility that is developed is fully coordinated with their efforts and needs.

I am wondering what plans for industrial coordination and participation you have.

Dr. **Shirley**. Mr. Chairman, we are interacting with industry in a variety of different ways—visits to industry, visits by industry, and so forth. We are very committed to input from industry on the formulation of the actual programs which will be conducted at NCAM.

We are planning to formulate these programs over the 6-year time schedule in which the center would be implemented. We would anticipate strong and continuous input from industry as we put these programs together. We are particularly interested in the intellectual participation of industry.

Mr. **Mineta**. That would be through the NCAM industrial fellows program?

Dr. **Shirley**. Yes, that would be one of the means of participation. There we would like to tie the participation of industrial scientists, if you like, at both ends. Suppose an industrial scientist came to work at NCAM—and we do have some experience and models for this already from experience at LBL. We would like to be sure that that scientist was not just coming on a sabbatical, as it were, but was coming to pursue research problems that would be of interest to his management and would be of mutual interest to a group at the laboratory, so that he could affiliate with a group to pursue research problems that would be of interest back home.

Mr. **Mineta**. I notice from your testimony that you have an NCAM advisory board, NCAM industrial fellows program, NCAM affiliates, which all have some industry participation in them.

What about in your ALS program advisory committee? Will that be primarily scientific from universities rather than industry?

Dr. **Shirley**. No, sir. It would have participants from both universities and industry.

We have experience with program advisory committees on our present national facilities. I think that the test of the sincerity of the laboratory in making the center a truly national center is that its program advisory committee must be dominated by people from outside the laboratory, as is the case with our existing program advisory committees. They actually have to make the recommendations for who is going to get beam time on the machines and how is that beam time going to be implemented. For that reason, we would like strong industrial participation.

Mr. **Mineta**. From San Jose, which, of course, right now is under water, but other than that, it is also the heart of the Silicon Valley, I know that there is a great deal of interest from the companies in our area about this. Without objection I will submit for the record a number of letters that I have received from companies in the area—Hewlett-Packard, Varian, Mobil Research and Development, IBM, Catalytics Associates, Inc., in terms of their own thoughts about this, and without objection, these letters will be made a part of the record.

[The letters follow:]

HEWLETT-PACKARD COMPANY
1501 PAGE MILL ROAD
PALO ALTO, CALIFORNIA 94304

JOHN L. DOYLE
VICE PRESIDENT, RESEARCH & DEVELOPMENT

February 15, 1983

Dr. David A. Shirley
Director
Lawrence Berkeley Laboratory
1 Cyclotron Road
Berkeley, California 94720

Dear David:

Many thanks for your letter of February 3 with the very pleasing news about funding for the National Center for Advanced Materials.

Hewlett-Packard strongly supports the establishment of the Center to strengthen the commitment for long-term basic materials research in focused areas of industrial need. In particular, we support the thrust in electronic materials research which will lead to a better understanding of the atomic structure underlying the synthesis, characterization and properties of both existing and new electronic materials. Such materials include semiconductor, dielectric, magnetic and polymer materials in both crystalline and amorphous states.

Applications for electronic materials place new and increasingly stringent requirements on material behavior. These include components such as semiconductor devices and integrated circuits, peripherals such as displays and memories and systems such as computation, measurement and networking applications. The requirements dictate the need for increased control over the structure, composition and properties of materials over scales ranging from the macroscopic to the atomic. In most cases, very little is understood about the defect behavior or compositional influence of these materials because they are relatively new. However, further advances in applications will be severely limited if such knowledge is not available from a well funded and sustained materials research program which recognizes the investment of skills, manpower, time and money required.

We strongly encourage NCAM in its efforts to build a substantial base in bulk crystal growth for substrate applications in opto-electronic and high-speed integrated circuits. In addition, expanded epitaxial crystal growth activities and concomitant characterization will provide important insights into the preparation of viable two- and three-dimensional structures at the atomic level where transport processes and energetic considerations play a major role. We will work with NCAM to plan for these basic program elements and develop a strategy for building up a center of excellence which will advance the state-of-the-art in synthesis and characterization of electronic materials. In the tradition of excellence at LBL, we look forward to the important education of scientists and technologists upon which our future depends.

David, many of us at Hewlett-Packard will be glad to work with the very significant research capability that is planned at NCAM, and I will personally be ready to help you get this project of national importance up and running.

Yours sincerely,

John L. Doyle

JLD:am

**Mobil Research and Development Corporation**

150 EAST 42ND STREET
NEW YORK, NEW YORK 10017

November 11, 1982

Professor D. A. Shirley, Director
Lawrence Berkeley Laboratory
University of California
Berkeley, California 94720

Dear Dr. Shirley:

Dr. Heinemann showed us background material supporting the Lawrence Berkeley Laboratory as the site for a major new research facility, the National Center for Advanced Materials. The scope of information to be provided by NCAM is broad and impressive, and it appears that LBL is uniquely equipped to manage this center. Furthermore, many of the objectives fit our needs well, and we enthusiastically support both the concept of the center and LBL as the host institution.

Some of the benefits that we foresee coming from a successful implementation of this project are listed in the attachment.

We are pleased that we can give you our wholehearted support.

Sincerely yours,

S. L. Meisel
Vice President, Research

J. J. Wise
Vice President, Planning

./jm
attachment

ATTACHMENT

## BENEFITS FROM A NATIONAL CENTER FOR ADVANCED MATERIALS AT THE LAWRENCE BERKELEY LABORATORY

1) The new light source can use the active spectrum and can be fine tuned to any part of it at intensities about four orders of magnitude greater than now possible. Pulse lengths of $10^{-11}$ seconds will permit observation of very unstable intermediates; e.g., on surfaces. A greater ability to split light beams to pencil beams of great intensity will enhance investigations of catalyst surfaces and species on them.

2) Advanced materials research requires both UV and x-ray sources, with the larger share being done in the UV and soft x-ray range (5-5000 eV) for which the ALS has been optimized. In addition, the ALS will be fitted with a high-field wiggler to provide a local source of hard x-rays up to 40 keV.

   With a spectral brilliance of $10^{18}$ photons per unit of phase space volume, the ALS will make possible a whole new realm of experimental studies. Transient species on surfaces, excited states of atoms and molecules, and atomic configurations during phase transformations will become accessible to experimental investigation, as will the properties of matter under extreme conditions. Of special interest will be the response of optical elements to high radiation fluxes. All of these studies will be further enhanced by the unique timing structure of the ALS, which will permit the study of atomic motion on the $10^{-11}$ second scale. ALS light can be used alone, in a two-channel configuration, or in combination with lasers to conduct very fast timing studies in a "pump-probe" mode. Thus, the ALS will be a central tool for the three research laboratories that form the other components of NCAM: The Surface Science and Catalysis Laboratory, the Advanced Materials Laboratory, and the Advanced Device Concepts Laboratory.

3) The Surface Science and Catalysis Laboratory will house programs covering a full range of surface science topics from the synthesis and characterization of highly surface-active materials and of new catalysts through the fundamental physics and chemistry of surfaces and reactions on surfaces to the design of new surface-dependent chemical processing techniques. Major areas of concentration will utilize the ALS, the transmission electron microscopes of the National Center for Electron Microscopy, and other state-of-the-art devices for the characterization of surfaces, interfaces, and of atoms and molecules adsorbed on surfaces, or segregated to grain boundaries. Magnetic angle spinning NMR, XPS, Auger, EXAFS are only a few available ones.

Synthesis of new materials of exceptional physical, electrical, chemical, and mechanical properties and modification and control of material properties will be important research objectives of the Advanced Materials Laboratory (AML). The ALS will be used in the characterization of material structure, surfaces, and research on responses to extreme environments.

4) The Advanced Materials Laboratory (AML) is intended for the interdisciplinary study of the synthesis and characterization of energy-related high-technology and strategic materials by use of the ALS and other means.

5) NCAM will be a national resource. Industry will be able and invited to participate in the design and use of ALS beam lines and ancillary NCAM laboratories to assure that NCAM basic research addresses America's high-technology needs. An NCAM Advisory Board, composed of scientific leaders from industry, universities and federal laboratories, will advise the laboratory director on NCAM scientific program directions. The NCAM affiliates, consisting of major research teams from industry and universities, will participate in the design, construction and use of ALS beam lines. An ALS Program Advisory Committee will review research proposals for the use of the ALS beam lines by all qualified experimenters.

6) Mobil scientists will be able to work with LBL personnel on the synthesis and characterization of new materials and particularly catalysts either by cooperation or by making use of an unparallelled combination of instrumentation for characterization and structure determination of materials which may also be totally prepared by Mobil in their own laboratories.

7) The new facilities will also lend themselves to investigate causes of metallic and/or ceramic structures and determine faults and potential or real causes of failure.

8) There are no limitations on the extent of personnel or time involved in use of the facility and no financial contribution is expected other than payment of Mobil's personnel and a user fee for expenses incurred by LBL.

9) Advantage can be taken of the assistance of the many outstanding staff and university members in many disciplines to attack specific problems.

**International Business Machines Corporation**

Thomas J. Watson Research Center
P.O. Box 218
Yorktown Heights, New York 10598
914/945-3000

February 16, 1983

Dr. David A. Shirley, Director
Lawrence Berkeley Laboratory
1 Cyclotron Road
Berkeley, CA 94720

Dear Dr. Shirley:

First of all, I want to congratulate you on getting your notion for a National Center for Advanced Materials accepted at the highest level of the government. I believe that strengthening materials research in this country is essential if we are going to retain our leadership in the physical sciences. I am also pleased to see that you are building on existing strength and in an environment where the chances of success are high. I shall be happy to provide any help that I can in setting up scientific programs in materials science that relate to the electronics industry. Approximately a year and a half ago I had carried out a study on materials research and its relationship to the present and future needs of IBM. I shall be happy to share some aspects of my results of this study with you. As far as the secondary objective is concerned, I am sure that at the least we will have scientists interested in collaborative research programs with people from NCAM.

I look forward to meeting with you during your visit here on March 7, 1983. I would like to schedule a talk by you to our scientists. If you think this is a good idea, I would appreciate it if you could send me a title and a brief abstract before February 28. I have also set aside time for you and me to have a more detailed discussion during your day here.

Sincerely yours,

Praveen Chaudhari

Praveen Chaudhari
Vice President, Science

PC/jra

cc: D. E. Ea[illegible]man

Catalytica
ASSOCIATES, INC.

February 18, 1983

Professor David A. Shirley
Director
Lawrence Berkeley Laboratories
University of California
Berkeley, California 94720

Dear Professor Shirley:

Thank you for sending me your proposal concerning the establishment of the National Research Center for Advanced Materials at the Lawrence Berkeley Laboratory. It is a well-conceived and intelligently integrated plan which makes a great deal of sense for the United States in these times of vigorous competition from other nations. I believe many business, government, and academic leaders are now recognizing the need for their coordinated efforts to keep America in the lead in areas of high technology. Your proposal will, in my opinion, be an important step in this direction.

As you know, we are a company specializing in the field of catalysis and are, therefore, very interested in the intellectual and instrumental capabilities which your proposed center would make accessible to us. As a relatively small company, we are unable to afford many of the sophisticated instruments which are vital to our research and, consequently, to the creation of new jobs through the future growth of our company. This is, of course, a personal example. I believe, however, that there are many small, medium, and large industrial organizations across the country which could benefit significantly from access to the facility that you propose.

It is my belief that materials science and, in particular, materials science as it relates to catalysis, is currently at a threshold from which there will be significant advances over the next 5-10 years. It will require interdisciplinary team efforts of sufficient size and using the most up-to-date instrumentation to achieve these advances and keep America in the lead in high technology areas. The center you propose is, in my judgement, vital in this regard and should be established without delay.

Your efforts and diligence in working for the establishment of the National Center for Advanced Materials at the Lawrence Berkeley Laboratories are appreciated by many and I wish you much success in the endeavor.

Sincerely yours,

J.A. Cusumano

JAMES A. CUSUMANO
President

VARIAN ASSOCIATES
EXECUTIVE OFFICES
PALO ALTO, CALIFORNIA 94303 (415) 493-4000

EDWARD L. GINZTON
CHAIRMAN OF BOARD

February 18, 1983

Mr. David A. Shirley, Director
Lawrence Berkeley Laboratory
1 Cyclotron Road
Berkeley, CA 94720

Dear Dave:

We at Varian are pleased to hear of your plans for establishing a Center for Advanced Materials at LBL. We share your views on the national importance of conducting research on the types of materials described in your plans. All too frequently the realization of significant practical devices and systems is frustrated by the inability to synthesize key materials, ranging from new classes of semiconductors to new types of superconductors.

Our contacts with Stanford, and more specifically our interaction with the scientific work at the Stanford Synchrotron Radiation Laboratory, leads us to concur in the importance attached to this type of high intensity light and x-ray source in your plans for the Center. I should like to underline the need for translating research and analysis into synthesis and control of new materials and devices to stimulate the attention of industry and thereby contribute to the nation's technological strength.

As a leading company in the semiconductor equipment field, and as a pioneer in developing the technology for and commercializing such important processes as ion implantation and molecular beam epitaxy, Varian scientists and engineers would welcome the opportunity to participate with LBL personnel in designing advanced equipment for the Center. We would also be interested in maintaining close contact with any device work in the microwave and solar photovoltaic fields undertaken at the Center. One suggestion on providing a mechanism for such interaction might be the formation of an Industrial Affiliates Group for the Center. Alternatively, contacts between individual scientists could provide informal channels for collaboration.

In summary, we endorse the concept outlined in the plans for the proposed National Center, and would like to be included as an industrial participant in the more detailed planning and ultimately in the technology transfer phase.

Sincerely,

E.L. Ginzt

E. L. Ginzton
Chairman of the Board

Mr. **Mineta**. Now, the concept of advanced materials is a new one for some of us, and we are used to hearing about our basic needs in the area of strategic materials. I realize that they are very different, but I wonder if they have any relationship either in general or in specific terms for the work that is contemplated at NCAM.

Dr. **Shirley**. Well, let me try to answer that by a somewhat generic example. One of the programs that we would like to emphasize in NCAM is related to catalysis. Catalysis uses strategic materials obtained from abroad such as rhodium, platinum, and other elements, the supply of which in the future may not be secure.

We cannot guarantee that we will find ways of replacing any given strategic materials, but we believe that a better understanding of these catalysts will lead to at least the possibility of strategic planning for ways in which one could substitute other materials for these strategic materials.

Mr. **Mineta**. I am wondering if you could share with us some of the practical applications, then, of a facility such as a national center for advanced materials. I am told, for example, that it would be of great use in the development of solar energy, and I am wondering how that would be so.

Dr. **Shirley**. Well, let me again, Mr. Chairman, give an example of a material that may well be important in connection with solar energy, namely silicon.

Some years ago, our laboratory decided that we were going to have to produce very pure germanium, which is close to silicon in its properties. Germanium used to be the most important semiconductor. Silicon now is. We had to do this in connection with nuclear detectors.

We developed a facility for producing and studying the purest germanium in the world. This led to some very nice basic research on the part of one of the scientists who is very much involved in the planning of NCAM, namely Eugene Haller.

We note that ultrapure silicon, which is now the most important semiconductor in connection with many applications, including solar energy, is available from only two sources in the world, and neither of them is a domestic source. Silicon Valley, needless to say, is critically dependent upon the supply of silicon, although not ultrapure silicon in general. You cannot say when ultrapure silicon will be of greater importance, but it is quite possible that in the future it will be important to have a guaranteed supply of ultrapure silicon.

One of the research programs that we are contemplating pursuing, if we have sufficient demand from industry, is the production—synthesis, I should say, and characterization and study of ultrapure silicon.

Mr. **Mineta**. Dr. Zucker, how does the ceramics research to be carried out at the High Temperature Materials Laboratory relate to similar work being carried out at NASA's Lewis Research Center?

Dr. **Zucker**. They are both parts of the same program. The program is funded out of the conservation part of DOE. Oak Ridge and NASA Lewis have divided up the work in a fashion that takes advantage of the experience of each laboratory.

Would you like to hear more about the coordination?

Mr. **Mineta**. Please.

Dr. **Zucker**. We started this work in June and looked at the overall question of structural ceramic development in the country. We visited a number of laboratories and visited NASA Lewis in September, and NASA Lewis scientists and NASA headquarters people came to Oak Ridge again in the middle of September.

Then, in December, it was decided to have bimonthly meetings between the NASA people and the Oak Ridge people, NASA Lewis and Oak Ridge staff. The first one of those was on the 9th of December. The last one was on the 25th of February. The first one was at DOE headquarters; the next one was at NASA headquarters.

So I think there is a connection, both at the headquarters level, and I know there is a good connection at the working level where the two groups of scientists visit each other.

If I may take a minute to add something else about the strategic materials that you raised. The nickel aluminum alloy whose properties I showed before—I am not sure you were here when that happened. Anyway, it is a very strong alloy that can supplant superalloys for many uses. Super alloys contain about 30 percent chromium and cobalt which all has to be imported from rather difficult places. Nickel and aluminum are much safer kind of material, and the nickel aluminides have that additional desirable property. No strategic materials need to be imported from unreliable sources.

Mr. **Mineta**. Dr. Richter, a question came up a little while ago about the nature of the work at SSRL. Is there not unclassified defense work taking place at SSRL?

Dr. **Richter**. I cannot answer that question for you. SSRL and SLAC are completely administratively separate. SSRL reports down through the campus and is not part of SLAC. We supply them with all their synchrotron light. It is a nice arrangement, but sometimes it leads to some bureaucratic complications like the present one.

There is no bar on Stanford's campus from doing unclassified work for any government agency. There is a lot of DOD funded work done at Stanford. I cannot tell you specifically what is being done at SSRL.

Mr. **Mineta**. Suppose the linear collider works as predicted and it is decided to build a larger, more powerful linear collider. Where would such a facility be built?

Mr. **Carney**. Brookhaven. [Laughter.]

Dr. **Richter**. I don't know how to answer that one, either. You are asking me questions that I cannot give you clear answers to.

I think it depends on two things. It depends on what energy you want and what energy per unit length we have been able to attain.

Right now the SLAC linac runs at an accelerating gradient of 20 million volts per meter of length. We have some R&D programs going on to up that to something on the order of 200 million volts per meter of length.

Now you have to pick the energy that the physics community needs. Suppose it was 1,000 GeV, like the Fermilab facility now.

Well, at 200 megavolts per meter of length, that needs 5 kilometers of linac. That 5 kilometers of linac could easily go in the Bay area.

Suppose you needed 50 kilometers of linac because what we were after is 10 TeV. Then one has to look for a site which can handle something straight and 10 kilometers long.

And so machines in the far distant future, I think, are going to have to move out of the existing laboratories. Each of our three big high-energy physics laboratories has only finite amounts of land, and the machines downstream, whether it is 10 years, 20 years, 30 years, those machines are going to be too big for our existing places, and we are going to new laboratories.

For the next step, they are clearly going at the present labs. For the step beyond the next, beyond SLC and Tevatron and CBA, they may or may not be able to go on the existing labs, and we are going to have to see.

Mr. **Mineta**. Is that something that ICFA is discussing at all, is that something that would be an international effort, or is that something that we would want to retain as our own?

Dr. **Richter**. I think it depends simply on how much it costs. If its costs are not too unreasonable, then I think we would like to maintain it as a national or a regional facilities. If the costs go completely out of sight, it is going to have to be a worldwide machine.

I, personally, would like to see our first step into international accelerators be more regional than worldwide. I think it would be easier for us to build a machine jointly with the Canadians and the Mexicans and the South Americans and perhaps even the Japanese than it would be to involve the Russians and the Europeans, also.

So I have always had this gleam in the eye for what I call a Pacific basin machine. Some day I would like to see it. I would also like to see it be built in the U.S., too. The shorter the distance you have to commute to your work the better off you are. [Laughter.]

Mr. **Mineta**. Has there been any discussion of that at all at CERN, for instance?

Dr. **Richter**. On building a machine with the U.S., there have been some very informal discussions on doing that. In fact, I started some of those in 1976. The reaction at that time from the Europeans was no; they didn't want to get involved. The Europeans were much richer in those days than the Americans were in high-energy physics. They still are much richer than the Americans are in high-energy physics, but they suddenly discovered that they are pressing their limits of funding very much, and I think they might be more interested now in thinking about international collaboration, but again, not for this generation of the machine.

You run into an interesting problem whenever you talk about this. Whenever you talk about this with the Russians, the Europeans, or the Japanese, it is, "Don't talk too loudly. We want to get out next national facility built; it is the one after that one should discuss to do internationally." That has been going on for 20 years now.

Mr. **Mineta**. The electric power costs for the SLAC facilities continue to escalate. I was wondering what kinds of measures are being undertaken to moderate some of those increased power costs.

Dr. **Richter**. We have a continuing conservation program going on in the Laboratory. We are trying to increase the efficiency of all our machines, the efficiency of converting power from the wall plug to particle beams. But an increase in energy of the machines is going to demand increased power.

We have started some informal discussions with Stanford University. The university is about to build a cogeneration plant which may have a surplus of electricity, and we started some informal discussions about participating in that.

Mr. **Mineta**. Well, let me thank the panel very, very much for their time and contribution in furthering our understanding of this whole field of our jurisdiction.

Mr. Carney, do you have any further questions?

Dr. Samios, are you prepared for some more questions from Mr. Carney?

Mr. **Carney**. No, I have a friend in Dr. Richter here.

Am I to assume that the further on we go with all these types of accelerators, and hopefully, we will continue to go further and further, to get to the types of energy necessary to continue to go further, you have to build a better mousetrap? Specifically, what I mean by bigger, I am not talking about energy; I am talking about the geography of it, you might want to say, the floor plan. Do they have to get bigger and bigger and bigger?

Dr. **Richter**. I think the answer is yes, and the reason I think that answer is pretty generally true is that the steps in energy that we know we want to take look to be bigger than the steps in acceleration efficiency that we know how to take.

For example, the proton machines are now being built with magnets of 40,000 or 50,000 gauss. You can go to 200,000 gauss, and at 200,000 gauss, the magnetic pressure is so high that the materials actually begin to flow, and you cannot make a magnet at bigger than 200,000 gauss if you are going to store a beam in it and run that beam around for a long time.

So there is a bound on the proton machine which is probably about a factor of four in size per unit energy, as long as you stick with magnetic techniques.

We are also beginning to play games with laser acceleration. I should not say plav games. We are beginning some research on making laser acceleration work. That promises, if it all works out, 1,000 MeV per meter, and it may be, if some of those things work, you can actually get the accelerators smaller. I am not terribly optimistic on making the accelerator smaller and the energy higher, but I would not say there is any physical principle that would prevent it, as long as you switch techniques radically. You would have to go away from magnets and go to lasers and plasmas to do the job.

Mr. **Carney**. I am going to ask a question that, maybe to a panel of such distinguished scientists, might seem funny, but I am just curious. As you build them, the one thing that I have seen with any one of these is the necessity for these things to be so perfectly accurate and perfectly level.

When you start talking about the machines and the magnitude that you described before, 50 kilometers, that type of thing, would you not run into an immense problem with the curvature of the earth?

Dr. RICHTER. No, you do not. The machines have to be straight or level only on the short range, not on the long range. If you take a machine that is 50 kilometers long, it has to be accurately alined, probably only over a region of several hundred meters, and you can bend it slowly. In fact, the magnet ring in the SLC is not level at all; it goes up and down hill by probably plus or minus 20 meters.

One of the machines at CERN, the new LEP accelerator. is actually tipped. It is not in a horizontal plane at all. SLAC points downhill.

There is a distance over which these accelerators have to be precisely alined and straight, but that distance is nowhere near the full size of the machine. The real thing to say about them is they have to be smooth rather than straight. You don't want sharp kinks in them.

Mr. CARNEY. And one last question that probably fits in that same category.

The machines now, when you are dealing with speed of the particle movement, they virtually all are going at the same speed, aren't they?

Dr. RICHTER. Yes.

Mr. CARNEY. And what you are trying to do is deal with energizing that particle more and more? Is that right?

Dr. RICHTER. Right. They are up against the relativistic limit. They are all very close to the velocity of light. You pour more energy in and they don't go faster; they just get heavier.

Mr. CARNEY. Heavier, all right.

Well, thank you, Mr. Chairman, and thank you for the response, doctor.

Mr. **Mineta**. Again, let me thank the panel for their participation today. We appreciate very much your being here.

The committee will then stand in recess until next Tuesday. Thanks very much.

[Whereupon, at 12:26 p.m., the subcommittee recessed, to reconvene on Wednesday, March 16, 1983.]

[This starts the 1984 Fossil Energy hearing:]

For testimony of

JAN W. MARES
Assistant Secretary for Fossil Energy
Department of Energy

before the
Subcommittee on Energy Development and Applications
March 8, 1983

please see

Fiscal Year 1984
Department of Energy Authorization
Vol. II, Conservation and Renewable Energy, Page

# 1984 DEPARTMENT OF ENERGY AUTHORIZATION

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WEDNESDAY, MARCH 16, 1983

HOUSE OF REPRESENTATIVES,
COMMITTEE ON SCIENCE AND TECHNOLOGY,
SUBCOMMITTEE ON ENERGY DEVELOPMENT AND APPLICATIONS,
*Washington, D.C.*

The subcommittee met, pursuant to notice, at 9:36 a.m., in room 2318, Rayburn House Office Building, Hon. Don Fuqua (chairman of the Committee on Science and Technology and the Subcommittee on Energy Development and Applications) presiding.

Present: Representatives Fuqua, Nelson, Boucher, Durbin, Bouquard, Bateman, Sensenbrenner, Winn, and Schneider.

Mr. FUQUA. The committee will be in order.

Today we continue our review of the fiscal year 1984 Department of Energy fossil energy research and development programs. Both today and tomorrow, testimony will be heard from nongovernment witnesses representing organizations with particular interest in fossil energy R&D.

The DOE program and its budget reflects the stated administration goal to redirect Federal funding toward long-term, high-risk technology base research. Under this concept, technology development beyond the proof-of-concept stage will be left to the private sector. We have been told that there are adequate incentives in the free-market system sufficient to initiate significant private sector research and development.

The Nation's budget deficit problem is still very serious, but also our energy situation is, too. Falling energy prices may stimulate the economy but the question is: will it depress the search for economic energy alternatives to oil imports?

The fossil energy budget request for this year is reduced by 59 percent below last year's appropriate levels and yet we are told that this is still sufficient to fund a well-rounded program, and maintain all viable fossil energy options. The committee must look at these issues very carefully, and we will be interested in the opinions of the private sector regarding the overall scope of the DOE fossil R&D program, as well as the proper public and private roles in energy research and development and demonstration of new technologies.

We hope to learn what will happen to the programs that have been, or will be, dropped by the DOE, and the question is: will the private sector pick them up?

Our first panel will be Mr. George Lawrence, president of the American Gas Association, followed by Mr. David Webb from the Gas Research Institute, and Mr. Bernard Lee, president of the Institute of Gas Technology.

Mr. Winn has a statement he wishes to make.

## STATEMENT OF HON. LARRY WINN, JR.

Mr. WINN. Thank you, Mr. Chairman, for this opportunity to welcome our witnesses this morning. We look forward to this testimony and discussion on the Department of Energy's fossil energy budget from witnesses outside of DOE. We certainly recognize a number of our witnesses, having worked with them over the years in our joint efforts to establish a meaningful fossil energy research and development program.

We all recognize that, in the past several years, there has been a significant change in the administration's energy philosophy. Earlier, we were very active in designing and building large powerplants and demonstration plants. Now, however, in recognition of our lack of basic knowledge, we have begun to devote our attention and our limited funds to a more basic and fundamental research, leading to some applied research.

This change in approach has been difficult for a number of contractors, and we realize that. We no longer are working to build bigger, but to delve deeper. Massive hardware has given way to bench scale equipment. The response to this change in priorities has been gratifying. We continue to look forward to the kinds of recommendations from industry which we see in today's testimony.

Mr. Chairman, I am certain that this hearing today will be helpful to all of us.

Thank you.

Mr. FUQUA. Thank you, Mr. Winn.

Mr. Sensenbrenner.

## STATEMENT OF HON. F. JAMES SENSENBRENNER.

Mr. SENSENBRENNER. Thank you, Mr. Chairman, for this opportunity to extend a welcome to our distinguished panel members. We look forward to their testimony on the fossil energy programs conducted by the Department of Energy. The administration has proposed significant reductions in these programs for fiscal year 1984. The administration proposes to fund only those research programs that may be termed high-risk, long-term activities that the private sector is unable to fund.

This philosophy is due not only to this country's economic situation and the willingness of the private sector to fund near-term research but also to a concern over the intrusion of the Federal Government into the marketplace. As representatives of corporations and research organizations that have been involved in the development of fossil energy, your thoughts concerning the underlying premise of the administration's proposals and the impact that prior programs have had on our economy and our energy supply will be appreciated and useful in determining the appropriate level of funding of these programs.

I look forward to hearing your testimony.

Mr. **Fuqua**. Thank you, Mr. Sensenbrenner.

Mr. Lawrence, we will be pleased to hear from you and, all of our witnesses—if you have a prepared statement, we'll make it a part of the record and, if you wish to summarize, certainly feel free.

## STATEMENT OF GEORGE H. LAWRENCE, PRESIDENT, AMERICAN GAS ASSOCIATION

Mr. **Lawrence**. Thank you, Chairman Fuqua, Congressman Winn, Mr. Sensenbrenner, Dr. Krebs.

I appreciate this opportunity; I will summarize very briefly and, if I may give something of an overview that I think will be logically followed by Mr. Webb and Dr. Lee, I think the gas-related research of recent years, as it has been proposed by the administration and how it is currently handled—I can't think of anything that better exemplifies some of the remarks that both of you gentlemen have made and that we need to be very cognizant of.

In the case of gas energy, for example, the Natural Gas Policy Act, with its phased removal of field price controls and incentives for new gas exploration, has made some rather dramatic improvements in conventional Lower 48-State natural gas supplies.

I think certainly with today's falling oil prices—and the oversupply we have—gas' competition with residual fuel oil in the industrial market makes it very easy for us to take our eye off the long-term ball.

And yet in the case of the gas industry and the contribution it can make, I think there have also been some very clear revelations as to what it can accomplish long term. We have the supply, resource base and potential—including new technology contributions. We've got a transmission and distribution pipeline delivery system that's the envy of the industrialized world. Practically all of the Western nations have a great appreciation for the increased contribution of gas energy that we have here, and they don't have anywhere near the delivery system to bring it about.

We have a research capability that's in place. It's making a contribution now. IGT is a longstanding institution. GRI was only formed in 1978, but it has a growing capability to make a tremendous long-term contribution.

I think we are seeing some movement by regulators—and we are a regulated industry in the transmission/distribution segment—but we're seeing a trend toward more sensible regulation to allow us normal business expenditures. And I think also, as inflation gets under control, it's a benefit to our regulated industry because it's a "given" that we don't keep up with inflation.

I think another point to keep in mind, too, as far as the gas industry is concerned, is that we really don't have some of the major public policy barriers to our contribution that other forms of energy have, whether they be imported oil, nuclear, or coal energy, or whatever.

So, in our long-term energy outlook that AGA has prepared—the energy supply outlook to the year 2000 and beyond—we find that we have a very realistic contribution of 5 trillion cubic feet, out of some 31 trillion cubic feet that's attributable to new technologies; and, in the years beyond 2000, that will grow.

I think the recommendations which the gas industry has made to this committee and to the Congress have been pretty austere in the past. We've made recommendations for our research only within the clear realm of our capability, and we are asking for an increase in fiscal 1984 over 1983 because we feel that we can now accommodate an increased capability.

The increase amounts to something in the range of $37 million more, or about 33 percent more than AGA recommended in fiscal 1983; this is about the same proportion of increase over the fiscal 1983 congressional appropriations, but it's over three times higher than the proposed DOE R&D budget.

DOE has made gas-related research one of its real targets for cutbacks. We think this is very unfortunate because we can think of no other area of research where the actual criteria that DOE has assigned fits so well. It's high-risk. It's high-potential in gas-related research. As we look at the areas which Mr. Webb and Dr. Lee will focus on, it's in the area of coal-gasification and unconventional natural gas where, again, the supply potential is huge and contributions are expected to be very, very major.

Fuel cells, conservation, and solar/biomass are areas where there's a virtually unlimited supply potential—if we can get the proper research breakthrough.

So, to that end, Mr. Chairman, Mr. Winn, we think we have very supportable recommendations; we have the research capability. We've been very prudent—even austere—in our recommendations in the past. They fit the research criteria. We think they have a real energy contribution to make to this Nation's energy policy goals.

Thank you.

[The prepared statement of Mr. Lawrence follows:]

# STATEMENT OF

GEORGE H. LAWRENCE, PRESIDENT
AMERICAN GAS ASSOCIATION

BEFORE THE

SUBCOMMITTEE ON ENERGY DEVELOPMENT AND APPLICATIONS

COMMITTEE ON SCIENCE AND TECHNOLOGY

UNITED STATES HOUSE OF REPRESENTATIVES

ON THE DEPARTMENT OF ENERGY FY 1984 RESEARCH
AND DEVELOPMENT BUDGET

MARCH 16, 1983

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## INTRODUCTION

Mr. Chairman and Members of the Subcommittee:

On behalf of the American Gas Association, I thank the Committee for this opportunity to testify. A.G.A. is composed of nearly 300 natural gas distribution and transmission companies serving over 160 million consumers in all 50 states.

A.G.A. urges this Committee to assure an appropriation of no less than $146 million for gas-related research and development in FY 1984. Adequate funding can be justified by scrutinizing the R&D funding criteria, the benefits of gas-related R&D, and the need for an R&D approach which strikes a fair balance between various energy sources.

## FUNDING CRITERIA

A.G.A. agrees with the Administration that the federal government should fund long-term, high risk R&D which cannot be undertaken effectively by the private sector alone. We believe that the specific projects for which we seek support qualify.

These projects are large scale and involve very sophisticated technology; thus, they require a number of years to progress from the experimental to the operational stage. Taken alone, the long-term nature of this research discourages private sector investment -- especially in a regulated industry.

Moreover, rate regulation imposes other disincentives. If a project is unsuccessful, the expenditures may be disallowed as "imprudently" made. This shifts the expense entirely to the shareholders. On the other hand, the benefits of a successful project are enjoyed only by ratepayers via reduced rates. Thus, the risk of receiving no return on R&D investment discourages devoting capital to such projects.

## BENEFITS OF GAS-RELATED R&D

Natural gas serves over half of both residential and commercial establishments in the U.S. and more of American industry than any other single fuel. Competitive price, cleanliness, availability, convenience and efficiency all contribute to consumer demand, which we believe is a barometer of gas desirability.

Further, gas provides a secure source of energy because foreign developments do not disrupt our supply. Greater recovery through improved technology will improve supply security for residential customers and further assure companies of the supply stability on which long term business decisions often depend.

## NEED FOR BALANCED RESEARCH

The Administration's FY 1984 budget proposals, if adopted, would devote a high proportion of funds to a fuel which, in the foreseeable future, will provide a low proportion of primary energy consumed in the U.S. That is, nuclear power consumption is quite low compared to other fuels, yet the Department of Energy budget proposal allocates over 50% of the R&D funds to this area. In contrast, fossil fuel supplies over 90% of U.S. primary energy use but receives only 6.1% of the R&D funds in the Administration's budget proposal.

In its action on the FY 1983 DOE budget Congress corrected a similar imbalance and appropriated $311 million for fossil fuels.

## RECOMMENDED GAS-RELATED RESEARCH PROJECTS

A. Coal Gasification: $50 million, minimum

The production of synthetic fuels will be a major contribution to the long term energy supply. Coal gasification, creating environmentally benign methane, can account for a major portion of this contribution. Although the U.S. is estimated to have vast coal reserves, up to 438 billion tons, only about half of these reserves can be recovered with current levels of technology. Technological improvements resulting from R&D, some of which are discussed below, will permit recovery of even more of our coal resources by expanding the breadth of coal feedstock that specific conversion methods can accept.

In-situ coal gasification could triple the nation's economically recoverable coal reserves by obtaining energy from unmineable deposits, but directional burning and long term controlled burning must be improved. However, the Administration proposes no funds for underground gasification technology development. Rather, it requests $2 million for environmental and advanced research, such as the controlled retracting injection point (CRIP) project. While these areas are important, other technology development would allow recovery of substantial coal reserves.

An active catalyst development program can yield supply benefits in two ways. First, it would provide a basis for the development of advanced coal conversion processes. Second, certain catalysts can accelerate chemical reactions within a gasifier and downstream processing systems, contributing to economical conversion processes and environmental control.

Pressurized ash agglomeration, which has been proven in large equipment with bituminous coals, could be extended to yield additional SNG from lower rank coals.

Support technologies for R&D will yield increased efficiency and production in coal gasification. Further studies and testing will improve gasification processes in the following ways: identifying contaminants in gasifier effluents; improving the understanding of the effects of high temperature and pressure on fluidization; determining the effects of contaminants in coals on the conversion process; reducing moisture and upgrading the heating value of low-rank coals such as lignites; and, adding data correlations to allow scientists and engineers to transfer empirical evidence into commercial application.

Coal gasification system component development would improve upon conventional technology, which uses process gas or steam to feed coal into high pressure gasification systems. This causes unavoidable loss of product gas, due to certain inefficiencies in compression systems. Better sealing equipment, such as a liquid sealed lockhopper system, would eliminate this loss of product gas.

Advanced gasification concepts (e.g., reductions in the size and/or number of equipment items in a conventional process) could improve conventional gasification process designs. In addition, gasification with carbon dioxide, rather than steam, offers system flexibility and more economical functioning. Full evaluation of this concept requires additional data.

Hot gas cleanup, via improved methods for removing corrosive acid-gas, could improve overall coal gasification efficiency and environmental acceptability.

Instrumentation and gasification development would permit continued research in several important areas. These include the examination of solids flow measurement and identification, improvement of devices for measuring erosion and wear (due to flow of solids) in transfer lines and metering components, and on-line analysis and metering of gases and vapor species.

B. Unconventional Natural Gas: $32 million, minimum

Gas from some of the sources discussed below can be extracted in rather small quantities using current technologies. More advanced methods of production will allow potentially vast additions to these resource bases.

Current production of gas from Devonian shales is limited to what producers can extract from around or below the shales, due to the low permeability of this type of rock. Improved techniques for "fracturing" the shale would allow gas to flow through it more readily, increasing recoverable supplies. Since only about 2-10% of this resource is producible at current levels of technology, advancements in "fracturing" may yield dramatic increases in this gas supply.

Tight sands gas is currently produced in relatively small quantities. Most of this resource base lies in lenticular formations -- long, narrow shafts reaching far down into the earth -- from which gas cannot be produced efficiently with current technology. Improved hydraulic, chemical and explosive fracturing methods would allow recovery of the 100-200 Tcf estimated to be economically feasible.

Advanced sources of unconventional natural gas, such as gas hydrates and abiogenic methane, may have tremendous supply potential if certain theories regarding their origin and extent are correct. Efforts to confirm these theories could yield an enormous supply payoff.

Large reservoirs of geopressured methane are thought to exist, especially along the Texas and Louisiana Gulf Coast. A substantial amount of these supplies (from 42 to 1200 Tcf) could be recovered with improved methods of brine disposal and development of materials capable of withstanding high pressures and corrosive elements encountered in drilling for this gas.

Methane from coal seams can be produced by hydraulic fracturing of the seam. However, high pressure combustion techniques, such as those used to link wells prior to in-situ coal gasification, may improve stimulation of methane production. Once the combustion techniques open channels in a coal seam, much higher long-term production rates are possible than if hydraulic fracturing had been used. Depending on the level of advancement in technological capabilities, among other things, recoverable gas from this source may range up to 60 Tcf.

C. Fuel Cell Technology: $20 million

By using an electro-chemical conversion process, fuel cells can save significant amounts of fuel used to generate electricity. The Administration requests no funding for gas-related phosphoric acid onsite technology development. Rather, the Administration's budget proposal would provide $3 million for electric utility R&D stack tests. A.G.A. believes that the completion of onsite technology development of phosphoric acid systems (along with other fuel cell technologies) merits R&D funding because private industry is not able to invest the capital necessary for this type of research.

Improved technology in fuel cells would also provide fuel savings incentives for installation.

D. Solar/Biomass: $20 million

Refined techniques for the conversion to methane of marine, terrestrial and waste biomass may yield enormous supply payoffs since biomass represents an inexhaustible, renewable energy source. However, to capitalize fully on this potential source of energy, conversion technologies must attain the efficiency necessary to compete with alternate sources of energy.

The gas industry has identified a number of projects that will aid the conversion of biomass to methane and, when perfected, will augment the nation's energy supply. For example, studies on the thermochemical conversion of biomass, the preparation for conversion of high-water content feedstocks, and genetic engineering will improve the efficiency of the conversion process. In addition, the establishment of biomass regional centers, for organizing and coordinating research activities, would enhance the advancement of scientific knowledge in biomass conversion.

E. Conservation: $19 million

The development of high-efficiency gas appliances and equipment can help to provide cost savings for the consumer while ensuring the most productive use of the nation's gas-energy supplies. Efficient utilization research directed at residential/commercial applications can make significant fuel savings in space conditioning, which accounts for almost 70% of this market. Research directed at the industrial market involves a number of processes in which improved gas-fired equipment can decrease product costs.

A.G.A. believes that conservation-induced fuel savings will contribute to improved energy source security and stability. These concerns merit serious consideration even though world fuel supplies are currently plentiful. Over-reliance on a foreign energy source, such as crude oil, may impair our national interests if foreign events abruptly cut off that source.

Areas of technology development that will reduce fuel consumption include the following: improving advanced gas prime movers and cogeneration equipment; increasing the amount of hydrogen that can supplement gas supplies without damaging pipelines; increasing the waste heat recoverable from certain industrial furnaces with thermochemical recuperators; and, natural gas vehicles.

Natural gas powered vehicles face a difficulty in their limited travel range. However, a two-phase development program can solve this problem. Near-term development of light-weight, high-pressure cylinders can facilitate expansion of fleet usage. Composite materials such as fiberglass -- steel or aluminum -- graphite, can reduce cylinder weight to approach that of a conventional gasoline tank of similar capacity. For the longer term, development of new storage technologies should be pursued. For example, solid adsorbent systems could increase fuel storage capacity per unit storage volume, while decreasing the required compressor output pressure.

F. LNG Test Facility: $5 million

A national test site for evaluating spills of various hazardous materials could provide the information necessary to improve the safety margin of handling such materials and reduce the cost of construction of new plants. The appropriation would fund both construction and data correlation of the spill test facility. The operating and maintenance expenses at the facility would not require future federal funding since the facility would impose user fees on those parties who require testing of materials. In this fashion, the private sector would absorb ongoing costs associated with the test facility, making federal appropriations a one-time event.

## CONCLUSION

A.G.A. believes that the recommendations made herein are for long-term, high risk projects and will lead to the successful completion of current programs that would not otherwise be funded by the private sector. Congress can provide a more balanced R&D program than the Administration's budget proposal by appropriating funds in line with these recommendations.

Further, all of the funds which A.G.A. recommends will be applied to research projects that will improve current technologies to the levels required to meet the nation's future energy needs. Since gas represents a source of energy for millions of consumers, the supply benefits of gas-related R&D funding will accrue to a very wide range of our populace.

A.G.A. urges Congress to consider the energy security for our country that will result from expanded supplies of domestic gas. Although alternative energy supplies are currently rather plentiful, we believe that the wise energy policy course to follow includes developing technology that will allow full exploitation of domestic resources, which are not subject to foreign supply disruptions.

Attached to this testimony is a list of the specific minimum funding levels recommended for particular R&D projects.

American Gas Association

### RECOMMENDED FY 1984
### DOE GAS RELATED R&D FUNDING PROGRAM
### ($ Millions)

| | | | |
|---|---|---|---|
| $ 50 | Coal Gasification | | |
| | $ 7 | | Underground Gasification (CRIP & Gulf) |
| | 43 | | Surface Gasification |
| | | $ 5 | Catalyst |
| | | 2 | Materials |
| | | 5 | Westinghouse |
| | | 1 | Pressurized Ash Agglomeration |
| | | 10 | Support Technology (studies, testing & basic r&d) |
| | | 3 | Component Development |
| | | 2 | Great Plains (administer loan guarantee) |
| | | 3 | Advanced Gasification Concepts (production & economics) |
| | | 2 | Eastern Shale Gasification |
| | | 3 | Entrained Bed Gasifier |
| | | 3 | Hot Gas Cleanup (corrosive acid-gas removal) |
| | | 4 | Instrumentation & Gasification Development |
| 32 | Unconventional Natural Gas | | |
| | | $ 6 | Tight Sands (flow characteristics of existing wells) |
| | | 6 | Devonian Shale (Eastern mineback, gas content & flow) |
| | | 6 | Advanced UNG (gas hydrates, abiogenic methane) |
| | | 12 | Geopressured Methane (additional well & instrument existing) |
| | | 2 | Methane from Coal Seams (new production techniques) |
| 20 | Fuel Cell Technology | | |
| | | $11 | Molten Carbonate (component design, test & analysis) |
| | | 6 | Phosphoric Acid |
| | | 3 | Advanced Concepts |
| 20 | Solar/Biomass | | |
| | | $ 9 | Biomass Conversion (reacter system & new processes) |
| | | 5 | Genetic Engineering in Biomass Growth & Harvest |
| | | 4 | Biomass Regional Centers (information dissemination) |
| | | 2 | Waste Conversion Technology (two phase process) |
| 19 | Conservation | | |
| | | $ 4 | Advanced Gas Prime Movers |
| | | 4 | Industrial Cogeneration |
| | | 3 | Hydrogen (production technology) |
| | | 3 | Thermochemical Recuperator (excess heat recovery) |
| | | 2 | Dessicant Systems (space conditioning) |
| | | 2 | Industrial Catalytic Gas Combustor |
| | | 1 | Alternative Fuels Utilization (Natural Gas Vehicles) |
| 5 | Safety | | |
| | | $ 5 | LNG Test Facility |
| $146' | TOTAL | | |

**AMERICAN GAS ASSOCIATION** 1515 Wilson Boulevard, Arlington, Va. 22209

Mr. FUQUA. Thank you very much, Mr. Lawrence.

Mr. Webb.

## STATEMENT OF DAVID O. WEBB, SENIOR VICE PRESIDENT, POLICY AND REGULATORY AFFAIRS, GAS RESEARCH INSTITUTE

Mr. WEBB. Thank you, Mr. Chairman and members of this committee.

I'm pleased to appear today and to have the opportunity to discuss the gas industry's views and recommendations that we consider essential for establishing a viable long-term, gas-related research program as an integral part of the Department of Energy's budget.

What we're here to discuss today is what we think is a true partnership between the private sector funding the more near-term research and a viable and a consistent policy in the Federal Government of funding the long-term research.

The need for a consistent Federal policy and adequate funding for long-term gas research is evident when you examine from where our energy supplies are going to be derived over the next 20 years.

There is wide agreement between both private and Federal forecasts that the United States will continue to rely on oil and gas for over 70 percent of our energy needs in the year 2000. While there's a short-term gas supply surplus now, all of these forecasts predict that, as we get closer and closer to the year 2000, more and more of the gas supplies must come from supplemental sources.

To develop these sources takes anywhere from six to 15 years, depending on the resource. In other words, we must start now. We must take the steps now to insure that the technology base is in place, so that when these resources are required they can be developed and brought into the marketplace by the private sector in a timely fashion.

The current softening of world oil prices, I think, provides us an opportunity at this time to develop the technology base in an orderly fashion rather than on a crash basis.

I think a policy of severely reducing Federal funding for the long-term R&D required to develop these new technologies at this time, in an attempt to solve short-term fiscal problems, mostly just restricts the development of new technologies and will actually provide little, if any, relief to the goal of significantly reducing the Federal budget.

Along those lines, the gas industry thinks that we need an R&D program in fossil energy of approximately $310 million for the total fossil energy program and, within that $310 million program, gas-related research at a level of approximately $90 million is recommended.

In the conservation R&D program, we are recommending that you should consider approximately $100 million total for conservation and, within that program, approximately $16 million should be devoted to gas-related research, primarily in the industrial sector where the opportunities are so great.

In addition, the gas industry is recommending $32 million for renewables and geopressured methane. These funding levels are consistent with actions taken by this committee and Congress in the past and, also, the funding levels are relatively consistent with what was appropriated by Congress in fiscal year 1982 and 1983.

More importantly, they will put in place a long-term technology base required to bring these sources online. In looking at whether our recommendations are reasonable or not, it seems there are three or four factors we ought to consider.

No. 1: Is it consistent with the administration's policy?

No. 2: Is the administration's budget consistent with its own policy?

No. 3: Have the gas industry and the manufacturing sector responded to the administration's new policy by funding more near-term research?

And, finally: Are there institutional barriers that should be considered in determining whether the gas related research recommendations that we are making today are consistent?

In the first instance, I'd like to quote a couple of statements made by Secretary of Energy Hodel in recent testimony. He stated:

> I believe that energy has an importance far beyond most commodities because directly and indirectly energy is significant to every aspect of the U.S. economy * * *

Going on, he stated:

> Another belief I hold very strongly is that overreliance on any one energy source is unwise and that a balance among energy resources is needed.

The gas industry and GRI agree with those positions. However, I think if you review the budget of the Department of Energy for gas research, it is not consistent with their own policy.

For instance, the gas-related research funding proposed for fiscal year 1984 in the fossil energy program is only $31.6 million, a drastic reduction from the $75 million approved by Congress just 3 months ago.

In other words, I don't think that their policy statements and the budget that they propose are consistent with one another.

In contrast to the inconsistencies in the Federal policy, if you'll look at how the gas industry has responded to, No. 1: increasing the size of their research programs and, No. 2: shifting to the near-term, I think you'll find that it has not only been responsive to the policy but it's a very impressive shift.

GRI will propose to its board of directors a 1984 budget of $150 million, an increase of 50 percent in just the last 2 years. Within this increase, this research is mostly concentrated in the near-term areas consistent with the Federal policy.

For instance, our funding for efficient utilization research, where the administration has stated that that's the industry's role, is slated to increase from $39 million in 1982 to a proposed level of approximately $68 million in 1984. So, of the $50 million increase, $30 million of it is directed at this area, which is consistent with Federal policy.

Efficient utilization research in 1984 in GRI will be approximately 54 percent of our total R&D budget.

Within the supply program, we've also shifted the emphasis on the near-term in response to the administration's policy. Our funding for supply research has grown from a level of $22 million in 1979 to a proposed level of $44 million in 1984.

However, within that program growth, funding for unconventional natural gas has grown from only 12.7 percent of the budget in 1979 to a proposed level of approximately 53 percent in 1984, again a shift consistent with the administration's policy and I think realistic with the marketplace realities.

One other factor that we have been very proud of is that we have significantly increased our industrial co-funding that we've been able to obtain from manufacturers and producers. That has grown from $8.8 million in 1979 to an expected level of $30 million in 1982 and we are projecting in 1984 a level of approximately $37 million.

However, one thing has become obvious to us over the last 4 years: This coordinated funding has really been directed toward the near-term research that can have a payoff in 3 to 5 years.

Therefore, we think it is consistent to back up the industry's increased emphasis on near-term research, budget growth, and manufacturing cofunding involvement, with a long-term research program of $146 million total within the Department of Energy's budget.

One other factor I think we should consider in determining the gas industry's recommendation is: Are there institutional barriers the gas industry has, or the utilities have that are unique in determining the amount of funding for research?

In order to try to answer that question, I'd like to read to you a brief extract from a recent study done by the Energy Research Advisory Board, which is the senior advisory body to the Department of Energy, in a report they submitted to the Deputy Secretary of Energy, titled "Federal Energy R&D Priorities." And I quote:

> A little over half our primary energy finds its way to consumers through the electric and gas utilities, and the utilities are regulated, price-controlled industries, selling their products not at free-market prices, but at controlled prices well below replacement cost. The consequences are that consumers of gas and electricity have less of an incentive to conserve and, therefore, to undertake research on conservation. . . . Both these regulated industries have weak incentives to spend on R&D. If successful, the benefits go to the ratepayers; if unsuccessful, the expenditures may be disallowed as 'imprudent'.

DOE, in its fiscal year 1984 budget, recognized these structural problems for the electric utilities by proposing a total electric-related research budget of approximately $1,450 million.

In contrast, with these same institutional barriers, the total budget proposed for gas-related research is only $45.5 million, less than half the $96 million appropriated by Congress in December.

While I recognize there are significant differences in the cost and the types of research for generating electricity versus gas, a ratio of over 30 to 1 is absurd. These deficiencies can best be corrected by providing a stable and viable long-term gas-related research program in the Department of Energy.

One other comment I'd like to make, Mr. Chairman, before going into some of the specific recommendations: The most immediate impact the decontrolled market will have on the gas industry's research program is to intensify the pressures to continue to increase our emphasis on near-term research, especially research on the more efficient utilization of gas.

The industrial market, which is very sensitive to fuel-switching based on price, will be the target of much of our increased funding for research in the next 2 or 3 years. We must develop the technology base to maintain the industrial market and this will put the continued pressure on this.

Therefore, I think this is just another reason why there must be a viable long-term research program in the Federal Government so we have a true partnership between the Federal Government and industry in this area.

Within the gas industry's recommended program, there are five important long-term areas of gas-supply research that I'd very briefly like to cover. The first is surface coal gasification. The gas industry is recommending $43 million, essentially the same level of appropriations as approved by Congress in fiscal year 1982 and 1983.

Within that $43 million, one of the programs that we think should continue to receive funding is the Westinghouse agglomerating ash, fluidized bed process. This is a joint program between industry and DOE. The DOE fiscal year 1984 money that's require is $6 million. Approximately $4 million of private sector cofunding has been committed. This money will be used to continue to do process-development unit tests, and to do some of the basic environmental work and the cold-flow test facility modeling work necessary for Westinghouse and the industry to build a large-scale demonstration plant that's been proposed for South Africa. Also, I believe there are one or two applications pending before the Synthetic Fuels Corporation which would use the Westinghouse process.

So, again, I think that's consistent. The Federal Government doing the basic research that allows industry to spend the $30 million to $50 million it's going to take to do these demonstration programs, which is the private sector role.

Another area that we feel that should receive funding in 1984 is underground coal gasification. We are recommending a funding level of $7 million, again, consistent with the appropriations of the last 2 years. Research on underground coal gasification, we think, should be continued by DOE to develop technologies capable of recovering unminable coal reserves in both flat and steeply dipping geometries.

One of the tests that we just completed last year under a joint program with DOE was a large-block test in Centralia, Washington where a new, promising concept proved to be feasible. We think that should be continued in 1984 with a large field test.

We also think there's much basic laboratory support work in insitu gasification technology that needs to be developed and we also think that they should start the initial investigations in testing the Eastern coals.

The third area in supply that we are recommending funding for in 1984 is unconventional natural gas research. We are recommending a level of $20 million. This is an increase over what has been recommended—excuse me—what has been funded in the past 2 years. However, it is consistent with the levels that GRI is putting into this program in the near-term areas.

We are proposing to our board of directors a funding level of $23 million in 1984 for near-term research in this area and we think that should be balanced with a long-term research program by the Department of Energy of approximately $20 million.

I'd like to read you just one quote from the administration's fiscal year 1984 budget request. They say:

> There appears to be adequate U.S. reserves of natural gas for several decades if conventional and unconventional gas resources can be exploited. Unconventional gas will not realize its potential unless there is an additional development of the scientific and engineering knowledge for associated extraction technologies.

That requires a joint program between industry and DOE in our opinion. The DOE program should have approximately $6 million to continue the multiwell experiment that was started last year in the Western tight sands.

We've had some recent studies done by Argonne National Laboratories on the potential in tight sands recovery. Their projections show that, if you could double the fracture lengths—the current technology is approximately 600 feet that you could fracture out into the formations—if you could just double that to 1,200 feet in just the tight formations alone, at a price of $4, which is the price we are talking about at the well head to be competitive in the market, you increase from 65 trillion cubic feet to approximately 110 trillion cubic feet the amount of gas that could be made available.

Therefore, the potential is very large; the research is long term and high risk. We also think the DOE program should have some funding in 1984 for the Eastern Devonian shale program.

In that program, we think the primary emphasis should be placed on developing an Eastern Mineback facility, which essentially is a combination surface-underground mine. There you can run experiments—actually go back and mine into the shale formations and determine what is happening in the formation—so that we can develop some computer models, develop some stimulation techniques that will allow us to double or triple the production from each one of these wells that have been drilled in the Devonian shale area.

Additional work should be done, we think, in 1984 by the Department of Energy in geopressured resources. Recently, an industry committee assessed the DOE program in view of the budget cuts and recommended that DOE complete the testing of the four design wells that have been drilled. They also said that the drilling of a new well should wait until these four projects have been completed and additional funding is provided.

We support those recommendations and feel that it is very important that DOE complete flow-testing of those four design wells so that, with the $70 million that has been previously invested, we can determine what the data is telling us about what type of research needs to be done.

One other area in supply that I'd like to briefly touch on is biomass, where we are recommending $20 million. We think that there should be patterned regional research in the biomass area. We've had such success in our joint program with the University of Florida's Institute of Food and Agricultural Sciences, where we put in half the money and the University of Florida puts in the other half of the money that we'd recommend that DOE look at patterning some regional programs based on this concept. We have access to the University of Florida's vast university system to do all kinds of basic research in biomass. This has been a very successful program and we think DOE should implement similar type programs.

We also think that DOE should provide about $5 million of funding in conversion technologies and about $9 million to continue work on silviculture test plots and biomass conversion studies with an emphasis on producing high Btu gas.

Mr. Chairman, I'd like to briefly touch on two programs in the conservation area before finishing: the fuel cell program and the industrial conservation program.

The gas industry is recommending a total of $20 million in the fuel cells budget for gas-related research. This would be over and above what would be requested by the electric utilities for electric-related fuel cells research.

GRI and the gas industry have made a major increase in funding for onsite fuel cell systems in the last 2 years. In 1983 and again in 1984, we will be funding approximately $20 million to offset the loss of DOE funds in near-term research in our previous joint program to field test these units.

We will pay for the total cost of manufacturing and field testing the 45 units, which is consistent with the federal policy and is an industry role.

At the same time, there is a need for a continued joint program in the long-term technology development to increase the reliability and lower the cost of these onsite systems.

DOE has initiated in 1983 a 4-year program for this research with the manufacturer, United Technologies Corp., and to meet their contractual commitments in 1984 requires approximately $6 million. The DOE work concentrates on the long-term research in fuel cell stack technology, the fuel processing system and catalytic research. GRI is funding a parallel program with UTC of approximately $12 million, concentrating on the more near-term technology such as component development, component fabrication techniques and design.

So what we are really talking about is a joint program with DOE and industry to continue the long-term research to bring the cost of these systems down. DOE should also continue long-term research on advanced fuel cell concepts, such as molten carbonate, where we are recommending $11 million, and solid oxides, where we are recommending $3 million.

Finally, we think the DOE program in industrial conservation should be continued and expanded. We are recommending $16 million in 1984. The reason is: there is a tremendous amount of opportunity in the industrial sector to do some basic research in engines, cogeneration systems, heat recuperators, ceramic materials, and burner systems. These have relatively near-term payoff and yet the industry and manufacturing sector is so diverse, they can't reap the economic benefits of generic research and, therefore, it's not being done unless it's done by the Federal Government or through a program like GRI's. GRI is funding in 1984, for instance, an industrial conservation, program of approximately $14 million, or essentially the same level we are recommending from the Federal Government.

Mr. Chairman, in summarizing, I'd just like to say: we feel that the recommendations we're making are consistent with the Federal policy. We think they are consistent with the programs that have been approved by Congress in the past. We think that they also recognize the structural problems that ERAB has pointed out regarding gas-related research funding, and we think that they also point to the need for a truly joint partnership between industry and DOE.

That requires consistent Federal policy, and consistent funding of long-term research while we continue to increase our research in the near-term area.

I'd like to comment very briefly, Mr. Chairman, on one other area: In reviewing the DOE fiscal year 1984 budget, several of the programs where Congress added money—not necessarily in gas-related programs but where they added money in fiscal year 1983,—the administration is proposing to use those funds in fiscal year 1984 for program termination costs.

So, where Congress is appropriating money to continue to do research or continue programs that have initially been started, the administration is proposing in 1984 in several instances to use funds to terminate programs.

I think that, from our perspective, increases the need for the committee not only to mark up legislation but, if at all possible, to have an authorization bill passed so that the intent of Congress is clear, and it's clear that these funds are to continue research—not to terminate projects.

Mr. Chairman, I thank you very much.

[The prepared statement of Mr. Webb follows:]

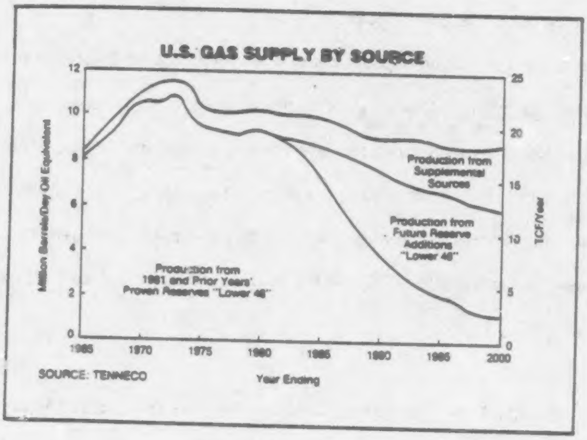
Statement of David O. Webb, Senior Vice President
Policy and Regulatory Affairs, Gas Research Institute
Before the Subcommittee on Energy Development and Applications
House Science and Technology Committee
March 16, 1983

Mr. Chairman and Members of the Committee:

The Gas Research Institute (GRI) is an independent, not-for-profit scientific research organization created by the gas industry in 1976 to plan, finance, and manage an expanded and fully coordinated gas-related research and development (R&D) program for the mutual benefit of the regulated gas industry and its ratepayers. In compliance with stipulated procedures, GRI submits its annual plan and five-year R&D progam to the Federal Energy Regulatory Commission (FERC) for review and approval as part of a public proceeding.

I am pleased to have the opportunity to appear before you today to discuss the gas industry's views and recommendations considered essential for establishing a viable, consistent long-term gas-related research program as an integral part of the Department of Energy's (DOE) FY 1984 budget.

The need for a consistent federal policy and adequate funding for long-term gas research is evident when you examine where the nation's energy supplies will be derived over the next 20 years. There is wide agreement between both private and federal forecasts that the U.S. will continue to rely on oil and gas for over 70 percent of its energy needs in the year 2000. While the U.S. has a short-term gas supply surplus now, more and more of the required gas supplies must come from supplemental sources as we approach the year 2000.



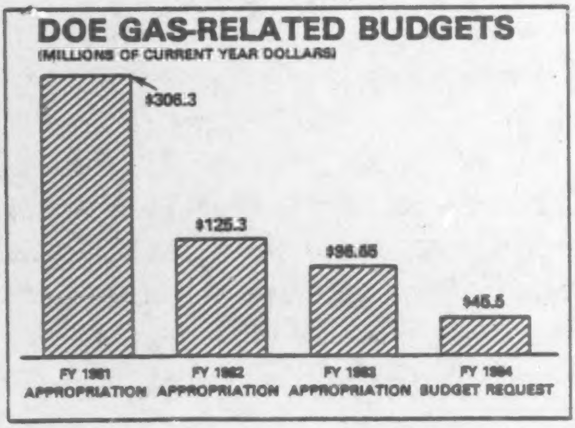
U.S. GAS SUPPLY BY SOURCE

SOURCE: TENNECO

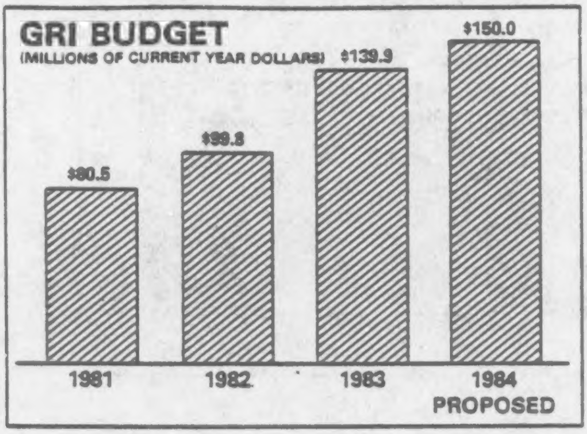
Establishing the technology base to develop these supplemental supplies at cost-competitive prices requires a lead-time of 6 to 15 years, depending on the resource. In other words, we must start now. If we don't take the steps necessary to develop the technology base required to use these domestic resources efficiently, the level of oil imports will rise rapidly, again increasing our dependence on insecure oil from politically unstable regions. The current softening of world oil prices provides us the opportunity to pursue this critical technology development in an orderly fashion rather than on a crash basis. To severely reduce federal funding for the long-term R&D required to develop these new gas technologies at this time in an attempt to solve short-term fiscal problems only restricts the development of critically needed new technologies and provides little if any assistance to the goal of reducing federal deficits.

Now that we have the time, we must seize this opportunity to establish and develop the required long-term non-nuclear R&D technology base by funding within DOE a total Fossil Energy Research Program for FY 1984 of approximately $310 million and a total Conservation R&D Progam of approximately $100 million, essentially the same funding level as appropriated by Congress for FY 1983. To maintain a minimum federally funded long-term gas research program required to supplement the expanded near-term gas R&D program currently funded by the gas industry, GRI is recommending a $146 million gas-related research budget. Within the Fossil Energy Program, GRI and the gas industry recommend funding for gas research at a level of $90 million. A gas research program of $16 million is recommended in the DOE Conservation budget and a $32 million budget is recommended for renewables and geopressured methane plus $8 million for hydrogen and safety. These funding levels are consistent with previous actions of this committee and Congress and will continue critical long-term research programs.

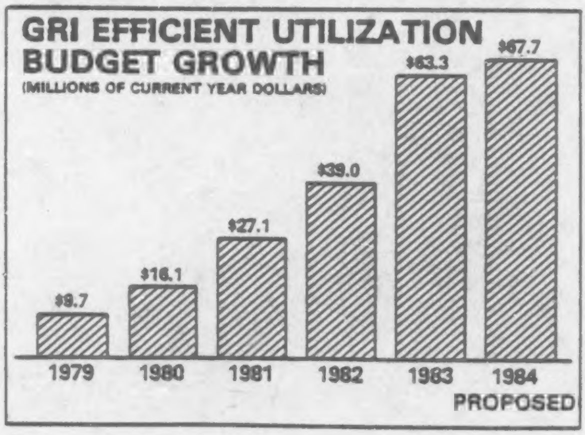
To illustrate the importance of providing a balanced and adquate federal gas research program, Secretary of Energy Hodel in recent testimony on the DOE budget stated, "I believe that energy has an importance far beyond most commodities because directly and indirectly energy is significant to every aspect of the U.S. economy . . . Another belief I hold very strongly is that overreliance on any one energy source is unwise and that a balance among energy resources is needed." GRI agrees with these positions. However, to implement this policy, DOE must continue to provide a stable funding base for long-term, high-risk gas-related research while industry is increasing its funding of near-term research. The budget proposed by DOE does not support this policy. The gas-related research funding proposed for FY 1984 in the Fossil Energy Program is only $31.6 million, a drastic reduction from the $75 million approved by Congress just three months ago. The Administration's proposal does not seem consistent with its own policy and certainly does not provide a stable funding base for long-term gas research.



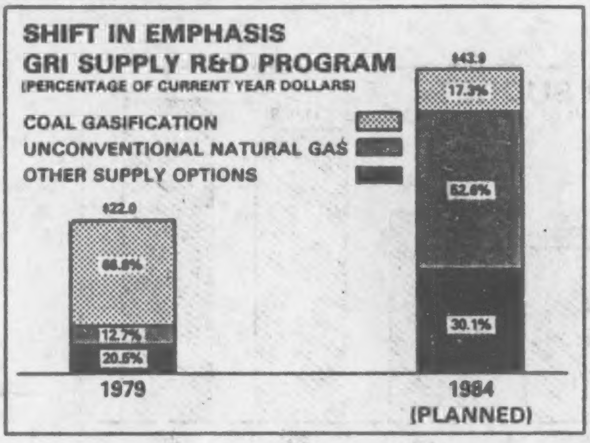
In contrast to the inconsistency in the federal policy for and funding of long-term gas research, the response of the gas industry to the revised government R&D policy has been rapid and very impressive. GRI will propose to its Board of Directors a 1984 budget of $150 million, an increase of 50 percent in two years, mostly for near-term research.



In response to federal energy policy and since it promises the most potential in the near term for ratepayer benefits, GRI's funding for research in efficient utilization continues to receive the largest budget increase from a level of $38.9 million in 1982 to a proposed level of $67.7 million in 1984 or approximately 54 percent of the total R&D budget.



Funding for the supply research program grows from a 1979 level of $22.0 million to a proposed level in 1984 of $43.9 million with the increase concentrated in near-term unconventional gas R&D.



GRI's emphasis on near-term research has significantly contributed to increased levels of coordinated funding from private industry. Industrial coordinated funding has increased from $8.8 million in 1979 to an expected $30 million in 1982. However, GRI's experience with industry cofunding has indicated an unwillingness to cosponsor research unless the payback potential is within five to eight years. The majority of coordinated funds are in areas that will be commercial in three to five years. Therefore, to provide a stable base of funding for long-term gas research DOE should fund approximately $146 million in the FY 1984 budget.

GAS TECHNOLOGY BASE

Another factor that should be considered in determining the proper federal role and the level of funding for long-term gas research is the institutional barriers that limit the regulated gas industry's ability to fund R&D. The gas technology base to develop cost-competitive supplemental sources of gas and more energy-efficient processes and end-use equipment required to compete with alternative energy sources is not established and in place. DOE's Energy Research Advisory Board (ERAB) in a report to the Deputy Secretary of Energy put these factors in proper perspective in its study Federal Energy R&D Priorities. I quote:

> A little over half our primary energy finds its way to consumers through the electric and gas utilities, and the utilities are regulated, price-controlled industries, selling their products not at free market prices, but at controlled prices well below replacement cost. The consequences are that consumers of gas and electricity have less of an incentive to conserve (and, therefore, to undertake research on conservation). . . . Both these regulated industries have weak incentives to spend on R&D. If successful, the benefits go to ratepayers; if unsuccessful, the expenditure may be disallowed as "imprudent." (Emphasis added).

In the case of electricity (but not gas) a substantial amount of R&D has been undertaken by large research-oriented equipment suppliers.

DOE in its FY 1984 budget request recognized this structural problem for the electric utilities by proposing a total electric-related research budget of approximately $1,450 million. However, they totally ignored these same institutional and structural deficiencies in funding R&D for the regulated gas industry. The total proposed gas-related research budget in DOE is only $45.5 million, less than half the $96 million approved by Congress in December. While there are significant differences in the cost of electric vs. gas R&D, a ratio of over 30 to 1 is absurd. This deficiency must be corrected by providng funding in the DOE budget for a viable long-term gas-related research program.

## DOE FY 1984 RESEARCH BUDGET
## ($ MILLIONS)

| RESEARCH AND DEVELOPMENT | ELECTRIC-RELATED* | GAS-RELATED |
|---|---|---|
| 1. CONSERVATION | $ 36.40 | $ 4.20 |
| 2. FOSSIL ENERGY | 44.95 | 31.60 |
| 3. SOLAR AND OTHER RENEWABLES | 54.90 | 9.70 |
| 4. MAGNETIC FUSION | 467.00 | – |
| 5. NUCLEAR FISSION | 848.00 | – |
| TOTAL R&D | $1451.25 | $45.50 |

*THIS IS A VERY CONSERVATIVE ESTIMATE OF THE REQUEST FOR ELECTRIC-RELATED RESEARCH AND DEVELOPMENT. NO PORTION OF THE $645.2 MILLION REQUESTED FOR GENERAL SCIENCE IS INCLUDED EVEN THOUGH MOST OF THE ENERGY-RELATED BENEFITS COMING FROM THIS RESEARCH WILL BENEFIT THE ELECTRIC UTILITY INDUSTRY. ALTHOUGH MOST OF THE ENVIRONMENTAL AND HEALTH RESEARCH AND DEVELOPMENT FUNDS ARE SPENT ON R&D RELATED TO NUCLEAR POWER, NONE OF THE $210.6 MILLION BUDGET REQUEST IS INCLUDED.

## IMPACT OF DECONTROLLED MARKET

The most immediate impact a decontrolled market will have on GRI's research program is to intensify the emphasis on near-term research, especially research on the more efficient utilization of gas. To retain key gas markets, particularly the industrial market which is very sensitive to fuel switching based on price, the technology base for industrial processes, equipment, and appliances that use or have the potential to use gas must be rapidly expanded. Just as GRI's past two budgets have targeted the increased funding toward near-term projects, I expect next year's budget to continue this trend. Therefore, a consistent federal policy and adequate funding for long-term gas-related research becomes even more important as the nation approaches a decontrolled market.

## RECOMMENDED GAS-RELATED RESEARCH PROGRAM

Without adequate continued federal funding for the long-term R&D needed to establish a sound gas technology base, supplemental sources of gas and more efficient processes will not become available at competitive prices within the time required to prevent permanent market loss to imported oil. Discussed below are five important long-term areas of gas supply research that are consistent with the Administration's policy guidelines and require continued federal sponsorship and funding.

## Gas Supply Research

| | FY 1982 Appropriation | FY 1983 Appropriation | FY 1984 Gas Industry Recommendation |
|---|---|---|---|
| SURFACE COAL GASIFICATION | $53.1 million | $39 million | $43 million |

One of the most promising, abundant long-term gas supply resources is the conversion of coal to gas. Of particular importance is the development of advanced processes that can use high-sulfur Eastern coals. For several years GRI and the Department of Energy jointly supported the initial development of several advanced gasification processes. In 1981, test results and technical and economic evaluations justified our concentrating on the further development of the Westinghouse agglomerating ash, fluidized bed process. Continued federal support in FY 1984 of $6 million is needed for this technology development. Funds will be used to continue the process development unit (PDU) tests, cold-flow test facility (CFTF), and laboratory research with a major emphasis on environmental aspects. GRI and industry will provide approximately $4 million of cofunding.

In addition, federal support is needed to continue basic research programs in improved catalysts, materials, components, and instrumentation since significant improvements in the economies of coal gasification can result from improvements in downsteam processing systems which currently comprise about one-third of the installed plant investment. A viable DOE program at levels consistent with the past two years' appropriations is necessary to continue the timely development of advanced coal conversion processes.

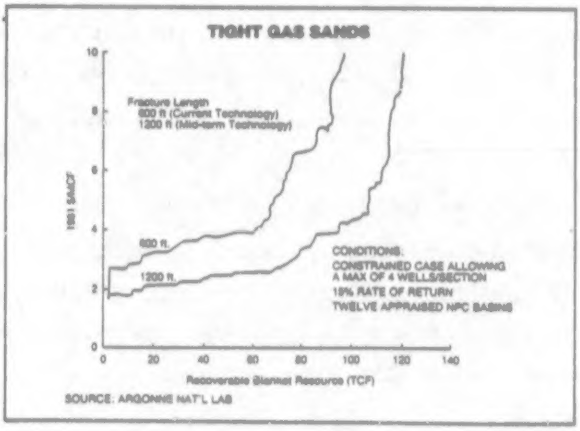
| | FY 1982 Appropriation | FY 1983 Appropriation | FY 1984 Gas Industry Recommendation |
|---|---|---|---|
| UNDERGROUND COAL GASIFICATION | $8.3 million | $6 million | $7 million |

Underground coal gasification offers the largest potential for major reductions in capital investment for production of medium-Btu gas or synthesis gas from coal, but the high risks associated with the requisite oxygen-blown underground gasification technology currently precludes industry from developing the technology on its own. Research on underground gasification must be continued by DOE to develop technologies capable of recovering unmineable coal reserves with both flat and steeply dipping geometries. Among the work completed under the DOE/GRI Insitu Participation Agreement was the large block tests at Centralia, Washington, in which the promising controlled retracting injection point (CRIP) technology was initially tested. GRI recommends continued development of this technology in a large field test as well as basic laboratory support of insitu gasification technology. Initial investigations in testing the Eastern coals should start.

| | FY 1982 Appropriation | FY 1983 Appropriation | FY 1984 Gas Industry Recommendation |
|---|---|---|---|
| UNCONVENTIONAL NATURAL GAS | $8.7 million | $13.9 million | $20 million |

With the U.S. confronted with unsatisfactorily high deficits and the gas industry facing a demand- rather than supply-constrained market, why should the Congress appropriate increased funds for long-term gas supply research in FY 1984? The reason is the lack of the required technology base. As the Administration states in the FY 1984 budget request: "There appears to be adequate U.S. reserves of natural gas for several decades if conventional and unconventional gas resources can be exploited. Unconventional gas will not realize its potential unless there is additional development of the scientific and engineering knowledge for associated extraction technologies." (Emphasis added). Long-term gas supply R&D is needed now because there is a ten-year lead-time from the beginning of research to the production of significant cost-competitive additional supplies.

The National Petroleum Council's (NPC) estimate of an additional 200 Tcf of reserves through development of advanced technology for tight lenticular formations and Devonian shales illustrates the potential and rationale for the high priority of this research. GRI is increasing its research budget for near-term work in these areas from $10.8 million in 1982 to a proposed $23.1 million in 1984. Since industry's focus in tight formations has been in blanket sands, DOE should focus on lenticular sands with a wide range of long-term technology issues. This is best accomplished through the continuation of the Multi-well Experiment at an FY 1984 funding level of $6 million. Continued support is needed in identification of continuity of lenses, long-term production testing, and down hole fracture diagnostics instrumentation. As an example of what can be accomplished by increasing the technology base, in a study done by Argonne National Laboratory for GRI, it was found that with current technology, projected tight sands recovery is approximately 65 Tcf at a price of just under $4/Mcf. If research can develop the technology to meet a mid-term R&D goal of doubling the fracture length, nearly 110 Tcf will become available at the same price.


TIGHT GAS SANDS

SOURCE: ARGONNE NAT'L LAB

In the Devonian shales program, the GRI program is focused on low-cost near- and mid-term reserve additions. The DOE program should address longer-term, higher-risk research at a funding level of $6 million. Priority should be given to establishing an Eastern Mineback Program consisting of a combined surface/underground test facility designed to substantiate the methodology and results of stimulation technology through a series of experiments. Researchers will have the capability to make actual physical measurements and detailed observations of the shale in place to determine the effect of stimulation, the behavior and extent of fracture, and the accuracy of diagnostic tools, among other factors.

Even though the potential resource base is huge, due to the very long-term nature of this research, GRI is only funding a small program in hydrates to investigate the various aspects of formation and disassociation of hydrates in porous media and the compositional and isotopic features of hydrates recovered from beneath the ocean floor on the Deep Sea Drilling Project (DSDP) and to measure the heat capacities of hydrates. Because of the long-term, high-risk but very high-payoff potential from generic exploration of this technology, a proper federal role should include research on physical properties of hydrates, hydrate recovery, gas production from hydrates, and the determination of the hydrate resources base. This will require $3 million to $4 million in FY 1984.

GRI is supporting another very long-term gas resource program that takes a broad look at all aspects and consequences of the abiogenic gas hypothesis. Gas samples are being analyzed compositionally and isotopically to elucidate their origin and migration history. A specific objective is to test the abiogenic methane hypothesis. Additional federal support of $2 million is needed for the formulation of critical tests on the abiogenic hypothesis.

| GEOPRESSURED RESOURCES | FY 1982 Appropriation | FY 1983 Appropriation | FY 1984 Gas Industry Recommendation |
|---|---|---|---|
| | $17.3 million | $8.4 million | $12.0 million |

While GRI's research efforts in this area have focused on near-term recovery of free gas trapped in reservoirs, the DOE program has focused primarily on the long-range recovery of methane dissolved in brine. Recently, an industry committee assessed the DOE program in view of the budget cuts and recommended that DOE complete the testing of the four design well projects. No new well tests are recommended until the current projects are completed and additional funding is provided. GRI supports these recommendations and proposes that a logical next step is to broaden the program to include a design well test in a hydropressured aquifer which may or may not include free gas. This test would provide information on gas liberation and transport, reservior and fluids properties, and artificial lift requirements to produce gas and brine. DOE has a legitimate and important role in R&D for enhanced recovery of gas from geopressured and hydropressured aquifers due to the long-term nature of the research and its potential high payoff.

| BIOMASS (GAS-RELATED) | FY 1982 Appropriation | FY 1983 Appropriation | FY 1984 Gas Industry Recommendation |
|---|---|---|---|
| | $3 million | $4 million | $20 million |

The unique advantages of biomass make it an excellent candidate for many regions of the United States that lack conventional energy resources. To provide a model for developing biomass energy programs on a regional basis, GRI began a program with the University of Florida's Institute of Food and Agricultural Sciences. DOE should expand the regional biomass concept by providing $4 million in the FY 1984 budget.

GRI is conducting research on plants, such as seaweed, which grow rapidly in the ocean and can be converted to methane. Marine farms, once developed, would not compete with valuable land space needed for food production. GRI is currently involved with projects at Catalina Island, the Santa Barbara coast, and in New York using kelp indigenous to each area to determine the economic feasibility of this concept and the kelp growth rates and subsequent methane yields. Federal funds of $2 million should be directed at basic research on increasing kelp yield through selective genetic engineering.

Federal support is needed in biomass conversion, growth, and harvesting. It is recommended that $5 million be committed to conversion technologies to include work with grasses and herbaceous feedstocks on a regional basis to allow resource assessment of the use of these crops for methane production, to investigate environmental effects of the removal or utilization of agricultural residues from fields, and to continue macroalgal growth studies without being confined to land-based systems.

In addition, we recommend $9 million be committed for continuing work with silviculture test plots and biomass conversion studies with an emphasis on thermochemical gasification for high-solids, high-lignin feedstocks with an emphasis on the production of high-Btu gas.

## Efficient Utilization Research

DOE has a continuing role in funding long-term research in the more efficient use of gas. Fuel cells and industrial processes offer great promise for significant improvements.

| FUEL CELLS | FY 1982 Appropriation | FY 1983 Appropriation | FY 1984 Gas Industry Recommendation |
|---|---|---|---|
| Onsite | $ 5.6 million | $ 4.0 million | $ 6 million |
| Molten Carbonate | 9.6 million | 7.5 million | 11 million |
| Advanced Concepts | 4.2 million | 5.0 million | 3 million |
|  | $19.4 million | $16.5 million | $20 million |

The gas industry and GRI have made a major increase in funding for onsite phosphoric acid fuel cell systems in 1983 (from $11.3 million in 1982 to $19.6 million in 1983) to offset the loss of DOE funds for near-term research. These funds will be used to continue the manufacturing and field testing of 45 units under the joint program with DOE. This total assumption by industry of funding for the field tests in 1983 and 1984 is consistent with national policy. However, the continued funding of the required long-term technology development program to increase the reliability and lower the costs of these onsite systems must be a combined DOE and gas industry responsibility. DOE has initiated a four-year program for this research with the manufacturer United Technologies Corporation (UTC) that requires $6 million in FY 1984. The DOE work concentrates on the fuel cell stack; the fuel processing system, including catalyst research; the inverter; and specialty heat exchangers. GRI has a parallel $12 million technology development program with UTC concentrating on component technology, component fabrication techniques, and system design. Together, these two research programs provide funding for the technology base required for the fuel cell to be developed.

Also, continued DOE long-term research is needed on advanced fuel cell concepts such as molten carbonate ($11 million) and solid oxides ($3 million). These systems, which could be field tested in the early 1990s, have the potential to be more efficient and to lower overall costs of energy. It is important that the government continue this long-term fuel cell research while the gas industry assumes funding for the near-term research and field testing of prototype onsite units.

| | FY 1982 Appropriation | FY 1983 Appropriation | FY 1984 Gas Industry Recommendation |
|---|---|---|---|
| INDUSTRIAL CONSERVATION | $10.5 million | $12.7 million<br>(4.0 million)* | $16 million |

*Represents specific gas-related projects.

There is a continuing need for federal funding of basic technology development in the more efficient utilization of gas in the industrial sector. As ERAB has reported, the manufacturing sector is so diverse that individual companies cannot capture the benefits of the R&D; therefore, the federal research role becomes critical. Specific technology-based research is needed on an advanced gas prime mover ($4 million), and industrial cogeneration ($4 million).

The problems associated with developing a small, economic and highly reliable gas prime mover for residential and commercial applications (heat pumps, cogeneration, refrigeration, and water heating) are numerous and difficult to solve. Funding and research will be required to meet stringent performance, life, and reliability requirements of the residential and commercial sectors. Major research needs include: dynamic component matching, improved high-temperature materials, ceramic development and application, better seal technologies, improved heat exchangers, better use of electronic engine controls, and high-temperature lubricants. A long-term federal research program to develop a small gas engine (prime mover) at a funding level of $4 million a year is needed.

Continued federal support is also needed for the development of improved combustion efficiency of industrial furnaces and boilers. Specifically, support is needed to develop an industrial catalytic surface combustor which would improve heat transfer rates and efficiency and reduce the combustor size. This would ultimately lead to substantial reductions in capital and operating costs.

Research support is needed for desiccant systems which are an attractive alternative to conventional air conditioning systems offering a potential fuel savings of 20 percent. The use of gas-fired desiccant systems for air drying shows good promise for meeting the needs for dehumidification in tight, well-insulated structures. Support of approximately $2 million is needed to focus on interfacing systems with typical building structures, other space conditioning equipment, and solar energy collector systems.

### Safety

|  | FY 1982 Appropriation | FY 1983 Appropriation | FY 1984 Gas Industry Recommendation |
|---|---|---|---|
| LNG SAFETY | $6.3 million | $3.0 million* | $5 million |

*It is unclear if funding to establish the Nevada test site facility is included in the FY 1983 Continuing Resolution. If, in fact, funding is committed in FY 1983 to establish the test site, only the $1.5 million is needed for the initial start-up activities in 1984.

GRI has been concerned with DOE's plan to terminate its LNG research program and close the test site at China Lake, California, where LNG spill testing has been conducted. GRI recommends the government establish a national test site in Nevada where large quantities of LNG or any hazardous gaseous material could be released and tested. It is estimated that $3.5 million is needed to establish the site and $1.5 million is needed for manpower and administrative costs. This is a one-time expenditure since future costs with the exception of $500,000 a year for manpower would be covered through user fees. Industry strongly supports establishment of this facility and the user fee concept for future operation.

SUMMARY AND RECOMMENDATIONS

A viable federal program consistent with Administration energy policy requires a budget in FY 1984 for a long-term gas research program of approximately $146 million (as detailed below). It should be noted that this recommendation is consistent with the level of funds appropriated by Congress for gas-related research in FY 1982 and is approximately the same as appropriated by this committee and Congress just three months ago for the FY 1983 funding level. (See Table 1).

## RECOMMENDED FY 1984 DOE GAS-RELATED RESEARCH ($ MILLIONS)

| | |
|---|---|
| **COAL GASIFICATION**<br>ADVANCED CONCEPTS, UNDERGROUND GASIFICATION, SHALE, CATALYSTS, MATERIALS, INSTRUMENTATION, FLUIDIZED AND ENTRAINED BED, SUPPORT TECHNOLOGY, COMPONENTS | $56 |
| **UNCONVENTIONAL AND GEOPRESSURED RESOURCES**<br>TIGHT SANDS (LENTICULAR), DEVONIAN SHALE, COAL SEAM METHANE, GEOPRESSURED, ABIOGENIC, HYDRATES | 32 |
| **BIOMASS**<br>DIGESTOR/CONVERSION, GROWTH, REGIONAL, WASTE | 20 |
| **FUEL CELLS**<br>ONSITE PHOSPHORIC ACID, MOLTEN CARBONATE, ADVANCED CONCEPTS | 20 |
| **CONSERVATION**<br>ADVANCED GAS PRIME MOVER, INDUSTRIAL COGENERATION, DESSICANT SYSTEMS, THERMOCHEMICAL RECUPERATOR, INDUSTRIAL CATALYTIC APPLICATIONS, ALTERNATIVE VEHICLE CONCEPTS | 10 |
| **HYDROGEN** | 3 |
| **LNG SAFETY** | 5 |
| | $146 |

TABLE 1

# DOE GAS-RELATED BUDGET
## ($ MILLIONS)

| | FY 1981 | FY 1982 | FY 1983 | FY 1984 BUDGET REQUEST |
|---|---|---|---|---|
| FOSSIL ENERGY | | | | |
| ADVANCED RESEARCH AND TECHNOLOGY DEVELOPMENT | $ 14.5 | $ 12.5 | $ 7.36 | $ 1.0** |
| ENHANCED GAS RECOVERY | 30.6 | 8.7 | 13.9 | 6.2 |
| ONSITE FUEL CELLS R&D | 9.5 | 5.6 | 14.0* | 5.5** |
| SURFACE COAL GASIFICATION | 164.9 | 53.1 | 39.0 | 16.9 |
| UNDERGROUND COAL GASIFICATION | 10.0 | 8.3 | 6.0 | 2.0 |
| GEOPRESSURED METHANE | 35.8 | 17.3 | 8.4 | 0.0 |
| ENERGY CONSERVATION | 22.0 | 10.6 | 4.0 | 4.2 |
| BIOMASS | 12.5 | 3.0 | 4.0 | 9.7 |
| LNG SAFETY | 6.5 | 6.3 | * | 0.0 |
| TOTAL | $306.3 | $126.3 | $96.65 | $45.5 |

*TEN MILLION DOLLARS OF THE FUEL CELL FUNDING IS FOR RESEARCH ON MOLTEN CARBONATE AND SOLID OXIDE FUEL CELL TECHNOLOGY. FUNDING FOR LNG SAFETY IS UNCERTAIN ALTHOUGH IT APPEARS THAT $3.0 MILLION WAS APPROPRIATED.

**NO FUNDING IS REQUESTED FOR ONSITE PHOSPHORIC ACID TECHNOLOGY DEVELOPMENT. THE FUNDING SHOWN IS FOR MOLTEN CARBONATE AND SOLID OXIDE FUEL CELL RESEARCH. MOST FY 1984 FUNDING FOR ADVANCED RESEARCH AND TECHNOLOGY DEVELOPMENT IS SHOWN AS PART OF THE COAL GASIFICATION BUDGET TO COMPENSATE FOR THE COAL GASIFICATION FUNDS BEING USED IN SUPPORT OF LIQUEFACTION APPLICATIONS.

Mr. FUQUA. Thank you very much, David.

And, now, Bernie Lee.

## STATEMENT OF DR. BERNARD S. LEE, PRESIDENT, INSTITUTE OF GAS TECHNOLOGY

Dr. LEE. Good morning. Thank you, Mr. Chairman and members of the subcommittee.

For the record, I'm president of the Institute of Gas Technology, a not-for-profit energy R&D organization in Chicago.

I would like to call first for a sustained policy for excellence in leadership in energy technology for this Nation and then maintaining that position once we get there. The absence of such a policy in the past has caused regular, predictable, disruptive, and wasteful oscillations in our efforts in energy development.

You look at other technologies—other fields. No one asks why we should be preeminent, for example, in national defense, because we would never think of importing major portions of our defense technology.

Our national commitment to the public health and agriculture has brought us the best health care system in the world and the most productive agricultural system in the world. Even though both industries are mature, well established, we continue as we should to fully support them at the Federal level in order to maintain our position of leadership. But we seem unwilling to strive for a similar leadership position in energy technology.

The central goal for our long-term energy policy should be to make our Nation invulnerable to the loss of energy imports.

Despite the current OPEC disagreements about pricing and production quotas, the oil cartel will stay and will grow strong again as soon as the world economy recovers. This is no time to slacken our efforts to develop those energy technologies needed for a stable future. We must continue to stress efforts to develop indigenous resources, and to use energy more efficiently.

To solve long-term energy problems, continuity in support is critical. We, as a nation, have already paid a costly and unnecessary penalty when, as a result of the lack of continuity in Federal energy R&D programs, many energy R&D organizations were forced to disperse the teams of highly trained scientific and engineering personnel that took years to assemble.

When the need arises in the future to again develop such teams, as it must do, more years will be lost before the new teams become effective. This lack of support continuity is one of the major roadblocks in achieving American preeminence in energy technology.

I feel energy is an area of national concern. The private sector could not, and should not, shoulder the major responsibility for planning and funding R&D in areas of national concern.

The Federal R&D budget for fiscal year 1984 shows steady support for public health and agriculture, and a huge increase for defense. Nondefense energy R&D is substantially reduced and, in particular, the nondefense, nonnuclear energy R&D budget is less than half of the fiscal year 1983 level, which itself was already minimal.

Especially glaring is the overwhelming emphasis on nuclear R&D at the expense of nonnuclear. This imbalance is totally out-of-step with the realities of the foreseeable future in which most of our Nation's energy supply will have to come from non-nuclear sources.

I, as a technologist, am very concerned about the R&D and commercialization of new technologies. The fiscal year 1983 budget directives stress support for research up to and including process development unit scale which itself fell short of bridging the gap to commercial acceptance.

Now, the 1984 budget directives further widen the gap to commercialization by instructing no funding for research activities past proof-of-concept at the bench scale. As a result of omitting such critical links in technology development, the Government will not receive an adequate return for its energy R&D investment of previous years. The private sector is unlikely to continue the development of a broad array of energy options once Federal support is interrupted because the private sector can only concentrate on the short term in view of the arbitrary and politicized nature of the world energy arena.

The private sector cannot risk its resources to strive for long-term stability when faced with the uncertainties and fluctuations in a world market controlled by different national governments.

The only effective route for long term U.S. energy stability is for the public and private sectors to work together on a sustained basis. Positive results through sustained Federal support are not only evident in public health, agriculture and aerospace, but also in the energy field—the nuclear industry, solar and photovoltaics and advanced fossil fuel conversion and utilization techniques.

For fiscal year 1984, to restore some balance between nuclear and nonnuclear R&D, I support the gas industry's recommendation for $146 million in DOE appropriations for gas-related R&D, comprising the following major categories: $50 million for gasification; $32 million for unconventional and geopressurized gas; $20 million for biomass; $20 million for fuel cells; $19 million for conservation and $5 million for LNG safety.

Now, I would like to take a few more minutes to discuss some specific recommendations in programs that IGT feels a strong support for.

Eight million dollars in coal gasification support research. To learn more about coal constituents and ash chemistry; to obtain fluidization data; to improve feedstock characteristics and to transfer R&D results to the technical community.

One million dollars to extend the ash agglomeration technique to higher pressures and to lower rank coals using an existing process research unit, capable of operation up to 500 psi.

Two million dollars to develop, in an existing facility, a CFFR process for extracting energy from oil shale fines, which are now discarded.

Twelve million dollar in geopressured gas R&D to continue production and testing of four existing wells plus participation in a new well to recover gas left in place after abandonment of water-drive gas reservoir.

Six million dollars in tight sands research including the continued effort in using a unique computer-operated rock analysis laboratory for core analysis, hydraulic fracturing and production testing of the wells already drilled.

One million dollars to initiate work to stimulate gas flow by relieving stresses in coal seams, using controlled and limited combustion to remove coal.

In the biomass area, $2 million to complete a test program on a thermochemical conversion of biomass to synthesis gas, using an existing pressurized fluidized-bed system built with DOE funds.

One million dollars to thoroughly study the first-known innoculum, which can biologically gasify woody biomass without pretreatment.

In the waste treatment area, $2 million for converting waste to fuel, including $1 million for two-phase biological digestion of municipal sewage sludge, and $0.5 million dollars for digesting landfills through the circulation of special bacterial cultures.

In the fuel cell area, $4 million to use the existing facilities to enhance a technology base of molten carbonate fuel cell, through the evaluation of components, prototype endurance, contaminant effects and electrolyte preparation.

Finally, in the conservation area, two promising approaches in industrial conservation, one for $3 million to develop thermochemical recuperation of high-temperature waste heat and the other for $2 million to develop an advanced catalytic combustor prototype.

Thank you very much for this opportunity to testify and I will be happy to answer questions.

[The prepared statement of Dr. Lee follows:]

Statement by Bernard S. Lee, President
Institute of Gas Technology
Before the House Committee on Science and Technology
Subcommittee on Energy Development and Applications
Hearing on FY1984 Authorization for the Department of Energy's
Fossil Energy Research and Development Program

March 16, 1983

Mr. Chairman and Members of the Subcommittee:

My name is Bernard S. Lee. I am president of the Institute of Gas Technology, a not-for-profit, energy R&D organization in Chicago.

I would like to make some observations of our national attitude toward energy R&D, and, in particular, toward striving for excellence and leadership in energy technology and then maintaining that position. I am deeply disturbed by the absence of a consistent and sustained national policy for energy R&D. This absence causes regular, predictable, and highly disruptive and wasteful oscillations in our technology development effort.

No one asks why we should be preeminent in national defense, because we would never think of importing major portions of our defense technology. Our national commitment to public health and agriculture through well funded and sustained efforts have brought us the best health care system and the most productive agricultural industry in the world. Even though both industries are well established, we continue, as we should, to fully support them at the federal level in order to maintain our position of leadership.

But we seem to be unwilling to strive for a similar leadership position in energy technology. Long-term energy policy should be directed toward controlling energy imports at a level at which the loss of such imports will have a minimal impact on the American economy and our national security. Despite the current OPEC disagreements about pricing and production quotas, the oil cartel will remain and will grow strong again as soon as the world economy recovers. This is no time to slacken our efforts to develop the energy technologies needed for a stable future. The only way to do this is to adopt a national energy policy which stresses the use of indigenous resources, conservation, and more efficient use of energy. To effectively carry out such a policy necessitates making a long-term commitment to energy R&D on a scale large enough to meet the goals.

To make any appreciable progress in solving long-term energy problems, continuity in government support for energy R&D is critical. Our nation has already paid a costly and unnecessary penalty when, as a result of the lack of continuity in Federal energy R&D programs, IGT and other energy R&D organizations were forced to disperse the teams of highly competent scientific and engineering specialists that took years to assemble and train. When the need arises in the future to again develop such teams, as it most certainly will, more years will be lost before the new teams become effective. The discontinuity of Federal energy R&D support is one of the major roadblocks in achieving American preeminence in energy technology.

Energy is an area of national concern. The private sector could not, and should not, shoulder the major responsibility for planning and funding R&D in areas of national concern. The federal R&D budget for FY'84 shows steady support for public health and agriculture, and a huge increase for defense. Non-defense energy R&D is substantially reduced, and, in particular, the non-defense, non-nuclear energy R&D budget is less than half of the FY'83 level, which itself was already minimal. Especially glaring is the overwhelming emphasis on nuclear R&D at the expense of non-nuclear. This imbalance is totally out-of-step with the realities of the foreseeable future in which most of our nation's energy supply will have to come from non-nuclear sources.

I am greatly concerned about current government policy regarding research and development and commercialization of new technologies. The FY'83 budget directives stress support for research up to and including process development unit scale, which itself fell short of the desirable goal of providing sufficient data for commercial development of processes by industry. Now the FY'84 budget directives further widen the gap to commercialization by instructing no funding of research activities past proof-of-concept at the bench scale. As a result of omitting such critical links in technology development, the government will be unable to receive an adequate return on its investment in energy R&D of previous years. Federal support must be continuous at a sustained level through the necessary stages of proof-of-concept, process development unit, pilot scale, and demonstration.

The private sector is unlikely to continue the development of a broad array of energy technologies once federal support is interrupted because the private sector can only concentrate on the short term in view of the arbitrary and politicized nature of the world energy arena. The private sector cannot risk its resources to strive for long-term stability when faced with the uncertainties and fluctuations of a world market controlled by different national governments. The only effective and feasible route to long-term U.S. energy stability is for the public and private sector to work together on a sustained basis. Positive results through sustained federal support are not only evident in public health, agriculture, and aerospace, but also in the energy field — the nuclear industry, solar and photovoltaics, and advanced fossil fuel conversion and utilization techniques. We must recognize that it is in our national interest to adopt a balanced and sustained policy for energy technology development.

For FY'84, to restore some balance between nuclear and non-nuclear R&D programs, I support the gas industry recommendation for $146 million in DOE appropriations for gas-related R&D. Specific recommendations for programs are summarized in Attachment 1 and detailed in Attachment 2 of this testimony. I will highlight a few areas below.

## Gasification

Of the $50 million gasification program recommended by the gas industry, in coal gasification the emphasis should be on supporting research that develops technology and assembles data that have broad applications. For this, $8 million is recommended to obtain data for the behavior of coal ash and coal constituents during gasification, to obtain process data on fluidization and gas cleanup, to improve feedstock characteristics, and to transfer R&D results to the technical community. A $1 million program is recommended to extend the ash agglomeration technique to higher pressures and to lower-rank coals using an existing process research unit capable of operation up to 500 psi.

In all current oil-shale processes, the fine particles produced during mining and feed preparation are discarded as a waste product. IGT recommends $2 million for the development of countercurrent free-fall retorting (CFFR) of shale fines using an existing bench-scale facility. This process is applicable to eastern and western shales, and has the potential for improved energy efficiency, resource recovery and environmental compatibility.

## Unconventional and Geopressured Resources

This $32 million program encompasses research in tight sands, Devonian shale, geopressured methane, deep-source methane, gas hydrates, and methane from coal seams. All these areas require much research and development before these tremendous energy resources can be tapped efficiently and economically. In particular, we highlight a new approach for enhancing the recovery of methane from coal seams which is theoretically more effective than hydraulic fracturing to stimulate gas flow. A $1 million program is recommended to initiate work to stimulate gas flow by relieving built-in stresses in coal seams via controlled and limited combustion to remove very small amounts of coal.

Using a unique Computer Operated Rock Analysis Laboratory (CORAL), the properties of cores from the first two tight sands wells of the multi-well experiment in Colorado were found to be different than anticipated. For FY'84 we support a $6 million effort which would include the continued use of CORAL in a comprehensive program of core analysis, hydraulic fracturing and production testing of the wells already drilled, along with the determination of gas entrapment and production mechanisms.

We support $12 million funding for continuing production and testing of the existing four geopressured wells through FY'84. Further, we recommend DOE participate in a new well to test brine production for recovery of gas left in place after abandonment of water-drive gas reservoirs.

## Biomass

The $20 million biomass program recommended by the gas industry includes research and development in the thermochemical and biological conversion of biomass, the conversion of waste, the growth and harvest of biomass and the establishment of several biomass regional centers. We highlight a $2 million recommendation to complete a RENUGAS test program for thermochemical conversion of biomass in an existing pressurized fluidized bed reactor system. On the biological conversion side, we highlight a $1 million recommendation to thoroughly study the first known inoculum which can gasify woody biomass without pretreatment.

## Fuel Cells

The $20 million fuel cell program recommended by the gas industry includes support for first-generation phosphoric acid fuel cell, second-generation molten carbonate fuel cell, and other advanced concepts. In particular, the molten carbonate fuel cell system has potentially the best combination of performance, efficiency, and cost to capture a significant portion of several end-use applications. A multi-year program with FY84 funding of $4 million is recommended to enhance the technology base which focuses on using existing facilities for evaluating components, testing prototype endurance, measuring effect of contaminants, and preparing electrolytes in a controlled environment.

## Conservation

The $19 million conservation program recommended by the gas industry includes a number of areas. We highlight two promising new approaches to industrial conservation. The first is thermochemical recuperation of high-temperature heat from industrial furnaces to preheat combustion air, for which a $3 million program is recommended to develop a sub-scale facility to evaluate design parameters, conversion rates, low-cost catalysts, and materials compatibility. The other is an advanced combustor concept for industrial applications that locates the heat exchange surface directly in the combustion zone and uses catalysts supported on these surfaces for combustion. A $2 million program is recommended to study a prototype design of this concept.

Thank you for the opportunity to present my views. I will be happy to answer your questions.

## ATTACHMENT 1
## TO STATEMENT BY BERNARD S. LEE
March 16 1983

For FY'84, IGT specifically recommends:

- $8 million for coal gasification supporting research
- $1 million for operating a pressurized ash agglomeration process research unit
- $1 million for component development
- $1 million for advanced gasification concepts
- $1 million for hot gas cleanup
- $2 million to develop the countercurrent free-fall retorting of fine-size oil shale
- $6 million for tight sands research
- $6 million for Devonian shale research
- $6 million for advanced unconventional gas research on deep-source gas and gas hydrates
- $12 million for geopressurized gas research
- $1 million for research using stress-relieving to stimulate gas flow from coal seams
- $2 million to complete work on the RENUGAS process research unit
- $2 million for converting high water-content biomass and wastes to fuel
- $1.5 million for the two-phase biological digestion of municipal sewage sludge and landfills to produce methane
- $1 million for biological gasification of woody biomass without pretreatment
- $1 million to initiate a regional biomass center at IGT
- $4 million for technology base enhancement of the molten carbonate fuel cell
- $1 million for hybrid hydrogen cycles evaluation
- $3 million for development of the thermochemical recuperative system
- $2 million for an advanced gas/solar desiccant system for heating and cooling
- $2 million for development of a catalytic surface combustor

### GASIFICATION

#### Coal

IGT recommends $8 million to carry out supporting research comprising: $0.5 million on the gasification mechanism of sulfur, nitrogen, and trace element contaminants contained in U.S. coals; $2 million for investigation of the dynamics of fluidized beds at high temperature and high pressure; $1 million on the behavior of coal ash during gasification; $0.5 million on the development of selective hydrogen sulfide removal using anhydrous organic solvents; $2 million for beneficiation of low-rank coals to reduce moisture and upgrade heating values; $1.5 million for compilation, evaluation and correlation of information relevant to all coal gasification processes in forms useful to engineers and scientists; and $0.5 million for bioprocessing of raw gasified effluent to remove sulfur and produce methane.

IGT also recommends:

- $1 million for extension of the ash agglomeration technique to higher pressures and lower rank coals using an existing facility

- $1 million for component development of a liquid-sealed lockhopper system for feeding coal into high pressure gasification systems, thereby eliminating gas compression and gas loss

- $1 million for advanced gasification concepts that utilize internal recirculation catalysts or carbon dioxide instead of steam as the gasifying agent to decrease the size and/or number of equipment items in any coal gasification process

- $1 million for hot gas cleanup using mixed metal oxide sorbents and other solid adsorbents to handle dusty and corrosive gases without conventional quenching.

## Oil Shale

The fine particles resulting from preparation of oil shale for processing are currently discarded as a waste product. IGT recommends $2 million for development of countercurrent free-fall retorting (CFFR) of shale fines using an existing bench-scale facility. This process is applicable to eastern and western shales, and has the potential for improved energy efficiency, resource recovery and environmental compatibility.

## UNCONVENTIONAL AND GEOPRESSURED RESOURCES

- IGT supports a $6 million program for tight sands. We propose a comprehensive program of core analysis, hydraulic fracturing and production testing of the depositional environments of the wells already drilled, along with the determination of the gas entrapment and production mechanisms of the wells.
- IGT strongly recommends $12 million for continuing production from the existing four geopressured wells with enhanced instrumentation and data interpretation in relation to natural gas in excess of that in solution in the brine. Further, we recommend DOE participate in a new well to test brine production for recovery of gas left in place after abandonment of water-drive gas reservoirs.
- For Devonian shale, IGT supports a $6 million program which would include our inputs to newly developed computer programs to gain a better fundamental understanding of factors that control gas content and gas flow.
- For advanced unconventional gas, IGT recommends a $6 million program for research on deep-source gas and hydrates and sediments, including means for measuring engineering parameters of hydrate deposits in nature.
- For methane from coal-seam, the DOE program should be expanded to include a new concept of opening horizontal channels in the coal to relieve pressure and stimulate gas flow, an approach that is potentially more effective than hydraulic fracturing. IGT specifically recommends $1 million to initiate work on this concept of applying limited combustion underground to remove sufficient coal to relieve stresses in the seams that will permit enhanced gas flow.

## BIOMASS

- IGT recommends $2 million to complete the research program on the RENUGAS process, in which a pressurized fluidized-bed process research unit has been built with DOE funding, to obtain fundamental data for the thermochemical conversion of biomass and wastes to fuel gas or synthesis gas.
- $2 million for research on two approaches to make fuel from biomass and wastes of high water-content, which are not conducive to direct thermochemical conversion.
- $1 million to establish the potential of an important breakthrough, the first known inoculum which can biologically gasify woody biomass without pretreatment.
- In waste conversion, $1 million to construct and operate a process research unit for two-phase biological digestion of municipal sewage sludge.
- $0.5 million for bench-scale research on LANFILGAS process for biologically digesting landfills through controlled recirculation of special bacteria cultures.
- $1 million to initiate a regional biomass center at IGT, one of several such centers that should be established to focus on candidate biomass and waste feedstocks for a particular region and to coordinate local research and interest groups.

## FUEL CELL

In the molten carbonate fuel cell area, IGT specifically recommends $4 million to initiate a multi-year program on technology base enhancement which focuses on using existing facilities for evaluating components, testing prototype endurance, measuring effect of contaminants, and preparing electrolytes in a controlled environment. The MCFC system has potentially the best combination of performance, efficiency, and cost to capture a significant portion of several end-use applications.

## CONSERVATION

- IGT recommends a $3 million program to develop a sub-scale facility of the thermochemical recuperative system to evaluate design parameters, conversion rates, low-cost catalysts, and materials compatibility. Such a system could significantly increase the waste heat recovered from high-temperature furnaces through preheating of combustion air.
- $2 million to initiate a program to build and test in the laboratory the H-COP MEC concept for residential heating and cooling. This is an advanced high-efficiency concept based on a gas-fired, open-cycle, solid-desiccant system that can be coupled with a solar energy source.
- $2 million to develop an advanced industrial catalytic surface combustor for firing package boiler and process heaters which locates the heat exchange surface directly in the combustion zone and uses catalyst supported on the surface for combustion. A program to develop this combustor, having broad national implication in energy conservation and emission control, would include fabricating a prototype combustor and measuring the performance characteristics of the heat exchange catalyst material and surface.

ATTACHMENT 2
TO STATEMENT BY BERNARD S. LEE
March 16, 1983

## GASIFICATION

### Coal Gasification

#### Supporting Research

o Fundamental information is needed on the gasification mechanism of heteroatoms such as sulfur, nitrogen, and trace element contaminants contained in U.S. coals. Isotopes of these heteroatoms can be used as labels to trace the conversion reactions and more accurately predict the particular contaminant species in the gasifier effluent. Such research will also provide the basic data needed to economically and efficiently process these contaminants in an environmentally acceptable manner.

IGT recommends $0.5 million in FY'84 to conduct research in this area.

o A critical need exists to investigate the dynamics of fluidized beds at high temperature and high pressure (up to 1000 psi and 2000°F). Fluidization data will be obtained on the potential synergistic effects of temperature and pressure on bubble size, bubble growth and the degree of solids mixing in order to fully understand gas-solids dynamics. Better definition of the combined effects of temperature and pressure will result in improved designs of fluidized bed gasifiers and the solids transport lines between reaction zones. Taking full advantage of a unique computer-assisted pressure probe analyzer, an existing fluidization test facility will be expanded as necessary to obtain data on a novel non-sintering oxygen distributor and its effect on jet penetration and the temperature profile within the jet.

IGT recommends $2.0 million in FY'84 to carry out this work.

o The behavior of coal ash during gasification strongly influences the reactor operability. Understanding the role of sulfur compounds and alkaline contaminants in coals on the formation of low-melting eutectics is essential to develop high carbon conversion gasifiers and high temperature dust removal equipment. Fundamental studies to monitor the changes in the physical and chemical properties of coal mineral matter under gasification conditions would employ existing equipment including bench-scale fluidized-bed reactors, a highly specialized pressurized hot-stage microscope, and an ash viscosity penetrometer.

IGT recommends $1.0 million in FY'84 to carry out this research.

o Highly selective hydrogen sulfide removal from coal-derived synthesis gas could significantly reduce the cost of sulfur management in coal gasification plants. Selective removal generates a highly concentrated stream which significantly improves conventional hydrogen sulfide conversion systems. Specific anhydrous organic solvents have been identified but require further development. These anhydrous solvents might also be used to clean up sulfur dioxide or hydrogen sulfide emissions from the sulfur plant tail gas, carbon dioxide vent gas, and steam plant flue gas. Using existing equipment, data will be obtained on the solubilities and heats of absorption of hydrogen sulfide and other synthesis gas components in the organic solvents at temperatures up to 2000°F, and pressures up to 1000 psi, over the range of gas compositions anticipated in gasification processes, to optimize the process.

IGT recommends $0.5 million in FY'84 to carry out this work.

o Beneficiation of low-rank coals such as lignites and subbituminous coals to reduce moisture and upgrade heating value will improve gasifier productivity and enhance raw gas quality with attendant capital, operating, and product cost reductions. Wet carbonization can be used to accomplish these goals. Existing PRU-scale equipment at IGT can be used to determine the effects of temperature, pressure, residence time, particle size, and feedstock characteristics on moisture reduction and heating value enhancement.

IGT recommends $2.0 million in FY'84 to apply this technique to low-rank coals.

o In order to effectively transfer the results of Government supported technology development to the point of commercial application by private industry, it is necessary to compile, evaluate, and correlate information relevant to all coal gasification processes in forms useful to engineers and scientists. Much of the available data is based on empirical relationships which can deviate widely from actual systems. Additional data correlations are needed on the properties of coal, char, ash and slag, synthetic sorbents, and related organic compounds and gas mixtures. Also, new data must be acquired on multicomponent vapor liquid equilibria combined with chemical reactions for design purposes.

IGT recommends a funding level of $1.5 million to collect and compile data in the areas listed.

o A novel low-pressure, low-temperature process to upgrade raw gasifier effluent streams is now under study at IGT. This process consists of two steps. In the first step, bacterial action is used to convert synthesis gas directly to methane, thereby avoiding conventional catalytic water-gas shift and methanation reactors. In the second step, anaerobic bioprocessing converts hydrogen sulfide directly to sulfur, thereby avoiding sulfur removal and regeneration equipment. Further development work of the total process at the laboratory and bench-scale level is required to establish the effects of reactor size and type, temperature, pressure, gas flow rate, and gas composition on conversion efficiency so that the optimum operating conditions of these two bio-reactors can be determined.

IGT recommends $0.5 million in FY'84 for research in this area.

## Pressurized Ash Agglomeration Process Research Unit

The technique of ash agglomeration affords very high conversion efficiency in producing gaseous fuels from coal. This technique has been proven in large equipment with bituminous coals. IGT proposes to extend this technique to higher pressures and lower rank coals. Using an existing 8-inch-diameter ash agglomerating PRU reactor, lignites and subbituminous coals can be tested at pressures up to 500 psi. High pressure is more favorable for the production of chemicals, methanol and SNG. This 8-inch process research unit is large enough to generate data that are reliable and closely simulate large-scale operating conditions.

IGT recommends $1.0 million in FY'84 to conduct research in this area.

## Component Development

Conventional lockhopper technology uses process gas or steam to feed coal into high pressure gasification systems. This results in rather costly and inefficient compression systems and unavoidable loss of product gas. A liquid sealed lockhopper system eliminates gas compression and gas loss. An existing PRU-scale water-sealed lockhopper system will be used to extend this technique to coal. Experimental data will be obtained to determine the effects of particle size, liquid medium, coal type and feed rate.

IGT recommends $1.0 million in FY'84 for research in this area.

## Advanced Gasification Concepts

IGT proposes two advanced coal gasification concepts that offer the potential of decreasing the size and/or number of equipment items in a conventional coal gasification process.

The development of an internal recirculation catalyst to enhance coal gasification kinetics in the range of 500° to 1500°F would result in a high efficiency process with low tar formation. Existing laboratory equipment has been used to identify several volatile catalysts which, with further development, could significantly improve conventional gasification process designs.

Gasification with carbon dioxide rather than steam offers the potential of system flexibility and more favorable economics. The range of available data needs to be extended to allow full evaluation of this concept.

IGT recommends $1.0 million in FY'84 to conduct this research.

## Hot Gas Cleanup

Downstream gas processing schemes that avoid conventional quenching and are capable of handling dusty and corrosive gases would improve overall coal gasification efficiency, economics, and environmental acceptability. Selective removal of corrosive acid-gas species and subsequent conversion to useful coal by-products can achieve these goals.

Several specific areas are immediately important. High temperature fuel gas desulfurization can be accomplished with mixed metal oxide sorbents that can produce sulfur directly upon regeneration, thereby avoiding sulfur dioxide scrubbing and/or reduction systems. Likewise, selective removal of carbon dioxide at high temperature can be accomplished with solid adsorbents that have been identified in exploratory laboratory experiments. Available equipment can be used to study the effects of gas composition, pressure, and temperature on removal efficiency and sorbent behavior.

These studies, coupled with a program to develop a water-gas shift catalyst that can function under conditions of high dust loading, combine to create a comprehensive program aimed at high temperature, raw gas processing that would avoid the costly and thermally inefficient quench step.

IGT recommends $1.0 million to carry out this work in FY'84.

## Oil Shale Fines Gasification

The fine particles resulting from the preparation of oil shale for processing are difficult to process for energy recovery and are normally discarded as a waste product. IGT is now directing its efforts toward developing an advanced gasification process which could utilize this potential energy resource. This CFFR process concept is the Countercurrent Free-Fall Retorting of fine particles of oil shale, a technique applicable to all oil shales, Eastern or Western.

The CFFR process is an energy-efficient and environmentally compatible method for increasing the overall shale resource recovery. The equipment required is simple and the gas produced is of high quality, therefore reducing downstream processing steps. For handling shale fines, IGT expects the CFFR process to be superior to fluidized bed or entrained flow retorting schemes. It could be used as a stand-alone process or as an adjunct to another process that cannot utilize fine particles.

IGT recommends $2 million for FY'84 for the development of the CFFR process concept, which can be initiated in an existing bench-scale facility.

## UNCONVENTIONAL AND GEOPRESSURED RESOURCES

### Tight Sands

Unconventional gas derived from the so-called tight sands refers to natural gas trapped in sandstone formations of variable porosities and permeability when compared with more conventional reservoirs. In FY'83, in a limited program, IGT analyzed core samples from the first two wells of the multi-well experiment in Colorado using our high resolution Computer Operated Rock Analysis Laboratory (CORAL). The matrix permeability of the rock, as accurately measured by CORAL, was found to be much lower than anticipated. However, initial production tests showed a much higher flow rate than would be expected on the basis of these matrix permeabilities. Some authorities have suggested that most tight sandstone production may be due to natural fractures. This is in strong contrast to prior conceptions and the assumptions in the multi-well experiment design. It is hypothesized that natural fractures may be responsible for the high initial flow rate of gas production from tight sands, which is followed by the much reduced flow rate due to matrix porosity and permeability. Also, greater gas content and permeability may exist in geologic zones between those previously targeted for hydraulic fracturing experiments. However, continued research is required on both in-situ gas content and in-situ permeability of sandstones with particular emphasis on better defining in-situ water content, which inhibits gas flow. For FY'84 we propose a comprehensive program of core analysis, hydraulic fracturing and production testing of the depositional environments of the wells already drilled. Additionally, determination of the gas entrapment and production mechanisms of the wells would be included in the $6 million program.

### Devonian Shale

The need exists for a better fundamental understanding of factors that control gas content and gas flow in Devonian shale. Modest electronic modifications of our computer-operated rock analysis laboratory provide the increased resolution and sensitivity required for measurements and analysis of reservoir properties needed as inputs to newly developed computer programs at METC which incorporate the fundamental factors. We look forward to active participation in this work as a part of a $6 million DOE program.

### Advanced Unconventional Natural Gas

In the area of deep-source gas, the various hypotheses as to the origin of such gas must be examined. Deep-source methane could originate from primordial methane trapped during the formation of the earth, or could be formed thermochemically from inorganic carbon and hydrogen in volcanic rocks. On the other hand, deep-source methane may be biologic in origin, but trapped deeply as material in the ocean floor is moved, or subducted, under continents due to plate tectonics. Much needs to be understood about this methane source because the implications of the earth's remaining methane resource could depend on the correct interpretation. Research efforts should be expanded to collect and analyze available geological, geochemical, and geophysical data to systematically assess the plausibility of various hypotheses. With improved knowledge of the origin of this gas resource, a much better understanding of how to develop it will result. This would be a cost-effective approach before field sampling and coring are undertaken.

In exploring methane hydrates, IGT's fundamental laboratory work has indicated that hydrates may have important roles in forming conventional gas reservoirs in both arctic and deep-ocean areas. But much additional laboratory research is required on fundamental properties of hydrates in sediments, and for development of tools to determine whether mobile fluids coexist with the hydrates. Substantiation of this would greatly improve economics of production.

IGT, therefore, supports a $6 million advanced unconventional gas research program on deep-source gas and methane hydrates in sediments, specifically including means for measuring engineering parameters of hydrate deposits in nature.

## Geopressured Methane

IGT strongly recommends $12 million funding for continuing production from the existing four geopressured wells through FY'84 and that additional and enhanced instrumentation and data interpretation be directed to more accurate determination of the gas water ratio. Further, we recommend DOE participate in a new well to test brine production for recovery of gas left in place after abandonment of water-drive gas reservoirs. IGT has identified locations where an estimated 4 trillion cubic feet of natural gas was abandoned in place because brine disposal costs could not be recovered at the then-existing natural gas price.

## Methane From Coal Seams

Fifteen years of mine safety research plus a modest DOE program during the last four years have clearly shown that hundreds of trillion cubic feet of potentially producible methane exists in coal seams in the U.S. However, with the exception of the Warrior Basin in Alabama, application of conventional gas well completion and coal mine drainage technologies have provided minimal incentive for risk capital investment by industry. Work at IGT provides the basis for a strong belief that the DOE program be expanded to include a new approach to exploit the energy supply from this source. IGT's research on gassy coal seams has revealed that opening of horizontal channels in the coal to relieve pressure could be more effective than hydraulic fracturing for stimulation of methane production. One possible means for doing this is by high pressure in-situ combustion techniques such as used to link wells prior to in-situ coal gasification. Once these horizontal channels are opened, gas pressure in the coal seams would then be reduced by producing the well, causing the flushing of combustion products from the channel, relaxation of stress in the coal seams, and much higher long-term production rates than possible with hydraulic fracturing.

IGT recommends $1 million in FY'84 to further work on this concept, by extensive laboratory analysis of coal core samples, coupled with computer simulation of stress relaxation in the coal. Such information would allow an effective planning of field tests which may be conducted independently or piggy-backed onto other related field tests.

## BIOMASS

### Biomass Conversion

o The IGT RENUGAS Process Research Unit (PRU) is a pressurized adiabatic fluidized reactor system built with DOE support. It is specifically designed and instrumented to obtain fundamental data for the thermochemical conversion of biomass and wastes to fuel gas or synthesis gas. To derive maximum benefit from this area of research, fundamental kinetics of biomass gasification must be obtained which will be combined with mass, momentum and heat transfer parameters of biomass gasifiers to develop computer-assisted process simulation models. These models for stationary and mobile applications will be validated with the RENUGAS PRU test results. $2 million is needed to complete this research program and identify trends in gasification characteristics, reactant requirements, and environmental aspects of thermochemical biomass gasification.

o For biomass of high water-content which are not conducive to direct thermochemical conversion, two promising approaches have been identified to make these feedstocks suitable for thermochemical conversion. One approach involves a two-step biothermal gasification process that first applies biological gasification to the feedstocks which not only produces methane, but also renders the residue suitable for the subsequent thermochemical gasification. IGT has initiated development of this process and further basic and bench-scale research is needed to define the key process parameters. The other approach is wet carbonization which improves both the dewatering characteristics, lowers the oxygen content, and increases the heating value of the feedstock. The wet carbonization is carried out at temperatures of 350° to 550°F and pressures high enough to prevent vaporization of water. Existing PRU-scale equipment at IGT can be used to determine the temperature, pressure, residence time, particle size, and feedstock characteristics on moisture reduction and heating value enhancement. IGT recommends $2 million in FY84 for conducting the necessary research in these two approaches.

o IGT has recently developed the first known inoculum which can biologically gasify woody biomass without pretreatment. This discovery should be further evaluated for a variety of woody species to fully establish the potential of this important breakthrough which could significantly advance biological converson of all woody biomass. IGT recommends $1 million to determine the effects on biodegradability and gas yields of feedstock composition and concentration, residence time, temperature, and inoculum type and concentration at the laboratory and bench scale. Genetic engineering techniques would be used to develop hybrid inocula which have high conversion capabilities. These inocula, once developed, would be incorporated into IGT's two-phase digestion process tailored to wood conversion.

### Waste Conversion

Disposal of wastes is a growing problem in all communities throughout the United States. IGT has been developing a two-phase biological digestion process than can be used to recover energy from such wastes while simultaneously stabilizing the residue and rendering it more suitable for disposal.

o In a current laboratory study IGT is testing its two-phase digestion process with municipal sewage sludge. The data to date indicate that over 5-1/2 cubic feet of methane can be produced for every pound of dry organic matter processed. A novel upflow solids digester is showing good promise as the digester type to be used for sludge. The next logical step is to construct and operate a continuous, adiabatic larger scale process research unit. Continuous on-site testing is necessary to prove the upflow reactor concept.

IGT recommends $1.0 million to construct and operate an on-site PRU to prove its two-phase digestion process for municipal sewage sludge.

o Two-phase biological digestion can also be used to recover energy from solid waste landfills. IGT's LANFILGAS process uses controlled recirculation of a specially developed bacterial culture to stabilize the organic waste within the landfill and to produce methane by two-phase digestion of leached organics in the recirculating culture. The process can be applied to large as well as small landfills. Small-scale laboratory data indicate the technical feasibility of the concept. Further data are needed at the bench-scale level to study the effects of culture recirculation rate, digester temperatures, and hydraulic and solids retention times in the acid-phase and methane-phase digesters.

IGT recommends $0.5 million to carry out bench-scale research on the LANFILGAS process.

## Biomass Growth and Harvest

Terrestrial and marine biomass represents renewable energy resources that have the potential for making a significant contribution to our energy base. Growth and harvesting have significant impact on the overall concept of using biomass for energy. Multiple-use species (where a portion of the plant is used for energy and a portion for food), such as sorghum and energy cane, should be studied to optimize their composition and yield. Consideration should be given to species with extensive root systems that prevent soil depletion. Development of woody species native to the northern states should be continued using short rotation farming techniques. These could provide a major energy crop base in those states where the yield of other biomass species are limited by colder temperatures.

IGT supports a $5 million effort for several research and development projects that would focus on optimizing crop yield and composition through genetic engineering, and on developing improved harvesting methods.

## Biomass Regional Centers

A number of regional biomass centers should be established which focus on candidate biomass and waste feedstocks for a particular region and which coordinate local research and interest groups, including government, industry, universities, and the general public. The centers would organize seminars, work shops, and conferences, and compile documents for dissemination of information. Since IGT has been in the forefront of all aspects of biomass development, from feedstock growth, harvesting, conversion, to assessment and information dissemination, $1 million for FY'84 is recommended to initiate such a regional biomass center at IGT.

## FUEL CELL

### Molten Carbonate Fuel Cells

Numerous comparative system studies have shown that the molten carbonate fuel cell (MCFC) has potentially the best combination of performance, efficiency, and cost to capture a significant portion of several end-use applications. Therefore, sufficient funding must be allocated toward developing the MCFC system to the stage where these projections of cost, performance, and efficiency can be reliably established.

IGT proposes a multi-year program, and recommends $4 million for FY '84 to further the program. Major tasks are as follows:

- Utilize existing facilities and capabilities to develop, test and evaluate innovative fuel cell components such as the electrolyte, anode, reforming catalyst for internal reforming cells, and the cathode and bipolar plate; endurance testing of bench-scale cells and stacks of cells; and carbonate management schemes and stack design. Using existing DOE facilities for testing prototype endurance, for measuring the effect of contaminants on cell performance, and for electrolyte preparation in a controlled environment would be cost effective and would provide impartial dissemination of information on cell life and performance directly to all potential manufacturers.
- Basic electrochemistry and accompanying support studies.
- Systems analysis of power plant designs and configurations, including performance modelling, gasifier selection, feasibility of anode gas recycle and establishment of fuel tolerance.

## CONSERVATION

### Advanced Gas Prime Movers

IGT supports a $4 million research program for developing advanced gas prime movers. There are many sound reasons for developing natural gas-fueled internal combustion engines. The high octane number of natural gas as a fuel provides superior anti-knocking characteristics which allow for high compression-ratio designs and thus highly efficient engines. Natural gas is a fuel that combusts cleanly, leaving no residual hydrocarbons, thereby greatly reducing the emission of pollutants to the air compared with conventional engines using liquid fuel. The clean burning characteristics of natural gas extend the life of the lubricant and the engine, leading to lower operating costs. Most importantly, natural gas is widely distributed and is much cheaper than liquid petroleum engine fuels. The broad deployment of natural gas-fueled engines will be beneficial to the nation in reducing its demand for oil.

One area of broad application is in small engines that provide on-site power generation and space conditioning in commercial and industrial installations. These engines, currently operated typically on diesel fuel, can be readily replaced by natural gas-fueled engines. These installations typically have natural gas service so that no fuel storage needs to be provided. IGT plans to actively participate in the development of a reliable and efficient small engine for natural gas.

### Thermochemical Recuperator

IGT has developed a waste-heat recovery system for high temperature industrial furnaces (for example, in the fiber glass and steel industries) that could significantly increase the waste heat recovered from these furnaces through preheating of combustion air. Currently available metallic recuperators are limited to about 1500°F in air preheat. Work is underway to develop ceramic heat exchangers to increase this to 2000°F. The IGT approach, however, uses no exotic ceramics. Instead, it recuperates the high-temperature waste heat, through a metallic heat recovery system, via the thermochemical conversion of natural gas with steam. The resulting synthesis gas mixtures, when combusted with air preheated by state-of-the-art technology, is equivalent to an air preheat of 2200° to 2600°F.

The benefits of the proposed TCRS (Thermochemical Recuperative System) are —

- Waste-heat recovery efficiencies exceeding 70% (depending on process conditions) as compared with only 30% to 40% for conventional recuperation.
- Fuel savings of up to 30% above that achieved by conventional recuperation.
- Reductions in flue-gas volumetric flow because of reduced fuel consumption which enable reducing other operating costs such as parasitic power requirements for stack ejector fans or using smaller furnace stacks. The reduction of flue-gas pressure drops may be as much as 50%.

- Increases in furnace production rate and improved production quality due to enhanced flame temperature and the higher combustion gases radiation that results from increased water vapor concentration in these gases.

- $NO_x$ emission abatement per unit of product produced due to higher heat flux to the product, higher thermal furnace efficiency, and an increase in furnace production rate compared with conventional heat recovery practice.

IGT recommends $3 million to develop a sub-scale facility of the TCRS to evaluate design parameters, conversion rates, low-cost catalysts and materials compatibility. The results will lead to a test program of a large scale prototype at an industrial site.

## Desiccant Systems

The past several years have seen the emergence of open-cycle solid desiccant processes as the most attractive alternative to conventional air-conditioning systems when coupled with a solar energy supply system. By design, the system is reasonably compact, simple, and inexpensive; utilizes no exotic materials or technology; and its operation is intrinsically safe and of potentially long life with moderate servicing.

An advanced gas-solar- or gas-only-fueled desiccant system, the H-COP MEC, was developed by IGT for residential heating and cooling. This design features a 95% efficient heat exchanger wheels and advanced reactivation design. The calculated seasonal "energy efficiency ratio" (EER) is twice that of commercially available air conditioning systems, and the projected cooling performance exceeds that of other systems now under development by more than 50%.

A funding of $2 million is recommended to initiate a program to build and test in the laboratory the H-COP MEC concept in a configuration for residential use, including gas-only and gas/solar firing, and the attendant desiccant technology development.

## Industrial Catalytic Surface Combustor

An advanced combustor concept, particularly applicable to package boilers and process heaters, has been developed which locates the heat exchange surfaces directly in the combustion zone and uses catalyst supported on these surfaces for combustion. Such a combustor would reduce the peak flame temperature substantially, thereby significantly reducing NOx formation, which is a principal pollutant in the flue-gas emissions. Furthermore, such a combustor would improve the heat-transfer rates and efficiency, and reduce the combustor size, all of which would mean substantial reduction in capital and operating costs.

This conceptual combustor design has broad national implications in energy conservation and emission control. To evaluate this concept, it is necessary to perform research and development on heat exchanger materials, preparation and treatment of the combustor heat exchanger surface, catalyst formulation and deposition, and catalyst life. After the laboratory research phase, it will be appropriate to fabricate a prototype catalytic surface combustor and measure its actual performance characteristics. $2 million is recommended for this program.

Mr. **Fuqua**. Thank you very much, Bernie, for a very interesting presentation—as a matter of fact, from all of our witnesses.

About 2 years ago, Bernie—I think it was the year before last, you testified that you thought by 1985 the price of oil would be about $100 a barrel.

Dr. **Lee**. No, sir. I think what I testified about was the total cost including externalities—the total cost to the American society, including the direct cost of oil, plus the externalities—that the Nation was already paying on the order of $100 a barrel.

Mr. **Fuqua**. Today.

Dr. **Lee**. Back 2 years ago, the societal cost, the cost of maintaining defense posture, the cost impact on our inflation, unemployment, all these factors.

Mr. **Fuqua**. But I thought you said the world price would be about somewhere in that neighborhood.

Dr. **Lee**. No, sir. I'm not prescient enough to make those projections.

Mr. **Fuqua**. Oh.

Mr. Lawrence, we hear a lot from oil industry executives saying that we've got to get the deficit down; there's too much spending going on. And I'm not in disagreement with that statement, but neither am I am in disagreement when you recommend and the other two gentlemen—that we have a responsibility to do more of the research work and gas technology, et cetera.

How do we answer that question when we are asked by not only gas executives but other people—say members on this committee? How do we justify spending all this money on something when the pressures are so great to reduce the deficit?

Mr. **Lawrence**. Certainly, we share the concerns of oil industry executives and members of Congress on getting the deficits under the control of this administration, but I think—you look at the record. You keep things in perspective.

We used to come before you and complain about the almost unfathomable disparity of electric and nuclear-related research of about 8 to 1 over gas-related research. And now they've expanded that to about 30 to 1 of electric-related over gas-related research.

And we look at the contribution that gas energy can make through our delivery system. We look at the potential. We talk a lot about our vast coal resources, and we've made various economic analyses to show that you convert these into high Btu gasification fuels, deliver them through our million-mile transportation system and it, in most cases, is going to beat how we use 75 percent of our coal today directly to generate electricity.

This potential is huge and we've got a whole laundry list here of very pointed research projects that can make our vast coal supplies into an environmentally compatible gaseous fuel.

You look at the tight sands formation—600 trillion—800 trillion cubic feet in the Rocky Mountain formation in the Devonian shale area.

And, to get that in perspective, we use about 19 trillion cubic feet today. You've heard Mr. Webb; you've heard Dr. Lee focus on some of the tight sands formation technology that is making a contribution. Right now, that's not economic. But the potential is huge.

And you go right on down the list. The contribution that gas-related research can make to this Nation's energy policy goals, certainly including its fiscal goals, are tremendous.

And we just, I think, are bearing the brunt of all this unfortunate austerity in the area that can really make the greatest contribution.

Mr. **Fuqua**. David, you mentioned that private funding for GRI had increased from about $8.8 million in 1979 to around a projected $37 million in 1984.

Mr. **Webb**. Yes, sir.

Bernie, how is it related to the institute?

Dr. **Lee**. The institute, as you well know, is an organization that does work under contract so we are encouraged by the attitude taken by the gas industry in supporting more R&D and we are just one of the contractors that does work for the GRI, but industry can only do so much.

And I think all we are saying here is a matter of balance. And I'm just as much a citizen interested in defense and all that, and in nuclear energy. But I think we are talking about the decimal points after the billions for the gas-related R&D.

Mr. **Fuqua**. Well, I think the point is that industry is spending more money.

Dr. **Lee**. Very much so.

Mr. **Fuqua**. Is this a result of the some of the reductions that have been made by DOE over the last two years that put the pressure on industry to increase their participation?

Mr. **Webb**. Perhaps.

I think mostly, though, it was in response to the gas industry's budget growing and having money to fund research in these areas.

I also think it probably has to do with the fact that we have, in my opinion, more flexibility and patent policies giving a guy an exclusive license—things that allow an individual or a company to see a way that he can get a payback on his R&D investment. This has happened in areas like development of the pulse combustion furnace, and in some of our industrial conservation work. These are areas where, in fact, there is a manufacturer that's going to take a product to market and we grant exclusive licensing in exchange for royalty agreement, so that we can get some payback on the investment. I think industry participation in those areas has been more in response to the gas industry's stepping up to its responsibility to fund more of this near-term research.

Mr. **Fuqua**. Institute has had collaboration with Argonne Lab. Have you had experience with any of the other national labs and, if you have, how has your experience been? How cooperative have they been working with an organization such as yours?

Mr. **Webb**. Mr. Chairman, we've had experience with Argonne, Sandia, Lawrence Livermore, and several of the operations offices.

Mr. **Fuqua**. Is that Sandia at Lawrence Livermore?

Mr. **Webb**. No. Sandia and Lawrence Livermore, as well as Argonne.

Mr. **Fuqua**. Which one of the Sandia labs was it?

Mr. **Webb**. Beg your pardon?

Mr. **Fuqua**. Which one of the Sandia labs? Los Alamos or Livermore?

Mr. **Webb**. It would have been Los Alamos who did work for us in Western tight sands. We've also done work with Oak Ridge in conservation and our experience has been very beneficial because what we're having those people do is use their unique equipment and facilities and capabilities. In unconventional gas, they took a lot of the knowledge they had gained in monitoring the underground nuclear tests and used that to help us develop some better instrumentation and some better techniques for monitoring our insitu coal gasification burns.

Oak Ridge has done work for us in the conservation area. We've worked in LNG safety with Lawrence Livermore. We've worked in data reduction with Lawrence Livermore. So what we try to do is take advantage of the unique scientific base that this Nation has in those national laboratories and use the basic exploratory scientific side of it to help us, if you would, in our near-term research.

Mr. **Fuqua**. Have you had any experience with the combustion lab at Sandia Livermore?

Mr. **Webb**. I don't believe we have, Mr. Chairman—not to my knowledge.

Mr. **Fuqua**. Have you?

Dr. **Lee**. No. We have no contract with them in combustion, no.

Mr. **Fuqua**. Bernie, you mentioned—and also Mr. Lawrence referred to—the imbalance among some of the energy technologies that we have.

How do you assess research priorities and where they should go in trying to allocate critical funding for programs? How do you allocate among the various technologies, recognizing that they all have a role?

Mr. **Lawrence**. Well, just very briefly—and I'll turn it over to Bernie—but I think we tried to follow the administration's criteria—high potential, the high risk that the commercial sector would not take over, but with a special focus on the high potential.

Dr. **Lee**. Yes; I think it's an extremely difficult job at first. As I stated, I feel energy deserves a much higher place as an area of national needs in relation to all the other national needs.

Once you've set that premise, I think that you can have a much clearer view about the long-term sustained nature that energy R&D must have. And then, once you've set the total size and period of time that you must have to keep the technology alive and growing, then the question is: how do you allocate funds within the energy sector?

And I think the smart thing to do is to apportion them according—at least use as a guideline—apportion according to the need. We don't think that gas-related R&D should dominate, but I think gas-related R&D should receive a proportionate type of a support in relation to its contribution to the supply of energy. That's all we're saying.

I think we need electricity research; we need nuclear research; we need gas research; we need oil research. Take them in a balance of more or less the current and the next 20-year projected distribution of energy sources and apply the support in accordance with that. I believe that would be a good guideline.

Mr. **Fuqua**. We've been trying to develop a synthetic fuel industry in this country for some time, without a lot to show for it.

There has been an on going oil-price reduction by OPEC. The Soviets are lowering the price of their oil almost $2 a barrel. There is also the possibility of Great Britain lowering the North Slope oil. What impact is all this having on our ability to develop these alternative technologies?

Dr. **Lee**. Well, if we are waiting for——

Mr. **Fuqua**. Including solar, conservation, and all the other technologies.

Dr. **Lee**. That's why I say: I think we need a long-term view—a sustained view. If we wait for—respond to the periodic ups and downs of the market—for that to decide what we should do in the long term. I think we are going to be misled.

Synthetic fuels or solar—any of these technologies require a considerable amount of time and sustained support.

I believe one of the background reasons for the OPEC adjustment of prices is to deter us from developing those technologies so that the world would be relying on the thing that they sell. There's nothing wrong with that psychology from their standpoint.

But, from our standpoint, I think just because prices move up and down momentarily——

Mr. **Fuqua**. That doesn't mean we should nibble on the bait.

Dr. **Lee**. Absolutely not.

Mr. **Lawrence**. Could I just add briefly, Mr. Chairman, it does cause us to take our eye off the ball and thank goodness, with the support of this committee, the Great Plains coal gasification project is underway. It's on schedule. It's on budget and that calls for, really, in effect, a regulated ceiling price of less than $39 a barrel of oil for synthetic, high-Btu coal gasification energy in this country.

There's only two of these in the world now. South Africa and now we've got one—will have. If you use Dr. Lee's total cost of imported oil of over $100 a barrel, I still think that's a valid consideration. But we are talking about an entity that will be in place for 25-plus years to produce that kind of energy.

Now, that might sound high today, when they've cut the price to $29 a barrel. But, in the late 1980's or the 1990's, $39 oil, coal gasification equivalent might be the best energy bargain any of us could get.

As a matter of fact, if we put in the coal gasification projects when they were originally proposed, without the tremendous delays—regulatory delays, primarily—we would have $4 a million Btu or $24 oil, coal gas plants on line.

Mr. **Fuqua**. I have a hunch that you're absolutely correct about 1990; $39 oil is going to look mighty good.

Mr. Durbin.

Mr. **Durbin**. Thank you, Mr. Chairman.

Let me say initially that, being from Illinois, I'm acquainted both with the Gas Research Institute and the Institute of Gas Technology. The fine work that you've done in that State is well known in the State capitol where I had worked for the last 14 years. I'm particularly glad to welcome Dr. Lee to this hearing this morning and for the help he's given my office and previous work I've done with Congressman Simon.

I'm interested, Dr. Lee, in your comment about Government looking for high risk, high-potential when they are determining where we are going to spend our limited dollars. Is there something that can be pointed to by the administration in terms of gas research as opposed to nuclear research or electric-related research which indicates that this is not as promising an area? Have we got a track record to point to in that direction?

Dr. LEE. Well, I can't say how the administration plans its long-term things but, if I were planning it, I would certainly make sure that all the links of the chain were adequately supported.

I feel the difficulty today is we carry a technology development up to a certain stage and, before we quite get there, we change our signals; drop back a notch and change our signals again, drop back another notch. The realities of life are that we must touch all the bases and develop all the chains; then, carry these technologies to the point of being accepted commercially.

Mr. DURBIN. Is there some sort of disparity in success?

What I'm suggesting is that we look at the history of this. Can we say that the nuclear industry has been more successful in their R&D than the gas industry?

Dr. LEE. Well, may I submit: I received my doctor of philosphy working in a nuclear field, so I have no bias toward or against nuclear.

Let me point out: Think back how many years we have been supporting the nuclear industry and today, even with a totally viable—we are recognized the world leader in nuclear powerplants—the bulk of the R&D for continuing developments on nuclear energy is still borne by the Government.

And the other fuel sources for the Nation—energy sources—have received nowhere near the type of support. Again, this is not to in any way denigrate the contribution of nuclear, but that's the way technology is and that's the kind of support it must have. The gas-related R&D has never received that sort of support in the past.

Mr. FUQUA. Mr. Lawrence.

Mr. LAWRENCE. Could I just add to that briefly? We go back a short 8 or 9 years ago to the then-Nixon administration proposal for what was virtually an all electric economy. There were going to be 1,000 new 1,000-megawatt nuclear plants. There haven't been any, but that was the energy of the future.

There was no contribution, long term, of gas energy whatsoever, so we were left out of their planning on research.

It's hard to reverse those trends. And now I think they are using arguments against us that some of the success we've had in getting rid of field price controls and improving the domestic supply of gas energy provide a recognition that, hey, this resource base is really already there—and therefore they persist in the argument that you don't need the gas-related research.

Mr. DURBIN. You state in your testimony, I believe, that this situation is a short-term phenomena—that is the surplus that we presently have on domestic gas resources.

Mr. LAWRENCE. Well, I don't mean to imply that too strongly. This so-called gas bubble or overdeliverability—it's been around for 5 or 6 years now, ever since we began to phase out the field price controls—but, if we do some dumb things, if we reimpose field price controls on new gas exploration, we'll get back into a natural gas shortage and then we'll certainly have to rely, even for the more immediate future, on coal gasification, and tight formation gas, and that sort of thing.

But this available supply of gas is there for the long term—both natural and new technology supply, in our opinion.

Our gas supply committee has looked at this in a very comprehensive way for over 3 years, and we've got our bulls and our bears on different sources of energy, but they come out pretty solid that that resource base is there and this long-term contribution is there.

Mr. DURBIN. Mr. Lee, you've got a quote in your statement that I find very interesting and I'd like you to elaborate on it.

It says, on your second page:

> Now that the fiscal year 1984 budget directives further widen the gap to commercialization by instructing no funding of research activities past proof of concept at the bench scale.

What do you mean by that term "gap to commercialization"? I think that might be important for us this morning.

Dr. LEE. Yes, well, we're developing technologies that we've been discussing this morning. We look at the conception of an idea—experimental work in a laboratory on a bench scale.

Typically, the next scale is a process development unit scale where you are beginning to see the integrated operation on a small scale. Then, we carry these technologies to a pilot plant where you run round the clock and try to get some large-scale, self-sustained operation data.

And, then, the next critical step is to build a so-called demonstration base where you are operating in a commercial operating mode—not an experimental mode—a commercial operating mode. The first commercial operation.

Then, by that time, the private sector will be in a position to evaluate both the economics of it, the environmental impact of it, and the technology aspect of the new technology.

So, an example that Mr. Lawrence alluded to—the Great Plains project—that's a technology that's been developed elsewhere in the world—certainly, to the commercial demonstration and, indeed, the commercial operational stage.

Had it not been done, we would not have accepted that as a commercial venture. What I am saying is that, in the last few years, we notice the chain being broken at an earlier and earlier stage, so now, in 1984, the directives are: We should only study technologies up to the bench scale; don't go beyond that, because that's for the private to take over. I disagree.

Private industry is not going to be in a position until the other links in the chain have been filled. And I appeal to the good senses of the people who make policy plans to realize that and to make proper adjustment for it.

Mr. DURBIN. Where does the Synthetic Fuel Corporation fit into this particular chain?

Dr. LEE. The Synthetic Fuels Corporation would at best—it is supposed to go one step beyond the demonstration. It is supposed to be supporting commercial operations, so the demonstration plant program, which used to be supported by the Department of Energy now actually is in limbo—no one is supporting, because SFC requires commercial criteria for success which a demonstration project obviously would not be able to meet. It's too big to be supported by the DOE and it's way too big to be supported by the private sector in light of the risk factor which I alluded to.

Mr. **Durbin**. We talked a lot about research this morning but, from what I hear now in your testimony, beyond that basic research, there is that gap between the bench research, the development of the technology and the role played by the Synthetic Fuel Corporation of the commercial application.

Dr. **Lee**. Very much so.

Mr. **Durbin**. I see.

As you are all probably aware, my district in central Illinois is part of the Illinois coal basin, which encompasses a fantastic amount of national resource in terms of potential. Yet, as I view it from year to year, I wonder if there is any aggressive effort being made at any level to develop these resources?

I'd like any one of you, or all of you, might comment on what you see as the potential—the future in the U.S. energy policy for Illinois basin coal.

Mr. **Webb**. Mr. Congressman, if I might, please. I think there is a potential there for it to be developed. I think the one thing that's needed is some sort of a policy by the administration.

My preference would be that DOE continue to stay in these joint ventures—for instance, our program with Westinghouse in coal gasification is to gasify Eastern coals and to use the coal fines.

That program needs a couple of more years to run so that some of the applications for the Synthetic Fuels Corporation and the commercial demonstration they are proposing for South Africa can actually get underway and we can correlate the data in a small unit to the actual performance data that we are getting in the larger units. You can then determine whether your scale-up factors are correct, whether your modeling work is correct, and whether your environmental factors are considered.

So you need a consistent policy, as Dr. Lee said, in DOE that, every year, doesn't change the policy of where the private sector has to pick up.

We've adjusted to the near term, and that's where our budget increases have gone but now the long term has moved four more decades down the road, if you would, and it's now bench scale.

Next year, it'll be test tube maybe or theoretical exporation. The other thing, though, is that the Synthetic Fuels Corporation has the legislative authority to take some initiatives that heretofore they've not taken. I mean they certainly could have a targeted solicitation for high-sulfur, Eastern caking coal, for instance. They have the authorities to enter into joint ventures, and even contractor-Government owned contractor-operated projects.

OK. With the current softening of world oil prices and with the growth worldwide in oil demand, and also in the United States, I think the Synthetic Fuels Corporation ought to reassess its role. I think that one of the best ways to try to achieve that would be to stop worrying about production quantity so much and worrying about trying to put in place a diversification of technologies and resources.

Mr. DURBIN. Yes, sir?

Mr. LAWRENCE. Congressman Durbin, I think there are two very compatible ways in which the gas industry can work to develop long term and short term your Illinois basin coal, and our other vast potential coal supplies. No. 1: we've had studies out in the street now for 6 years that show, from the standpoint of economic efficiency, consumer impacts, environmental impacts, and resource efficiency, that high Btu coal gasification beats coal electrification.

That—in our long-range projection, coal gasification is 10 percent of our supply projection to the year 2000. Beyond, it could increase. That's the long-term.

The short-term. We think there's a tremendous opportunity to use more coal now in what we call the select use of coal, together with gas, where you use the environmental pluses of gas energy, with coal, in dual capable facilities that would burn both coal and gas. You would use the coal in times when it would have little environmental impact and combine it with more or less gas at other times. But you'd balance it.

It would be about 80 percent coal, 20 percent gas. This select use of gas would enable the use of more coal today in industrial facilities, consistent with our environmental standards. Now, we ought to pursue that.

Dr. LEE. If I may say a few things about the Illinois coal, we are quite familiar, as Mr. Webb said—the Great Plains project is one based on lignite, noncaking, low-grade coal.

There is no technology available commercially for handling highlycaking bituminous coal and, being from the State of Illinois, we have tested—if I remember correctly—Illinois No. 6 seam from Peabody No. 10 mine somewhere.

Mr. DURBIN. Somewhere 20 miles south of my home.

Dr. LEE. Is that right? I've not been to the mines, but we've received a lot of coal. It's a mildly caking coal.

And we also tested coal from a Sahara mine. I remember, in No. 6, a highly caking coal. We have tested run-of-mine coal, unwashed because that, again, improves the utilization efficiency of coal if you don't have to lose 25 percent of the heating value by washing the coal to upgrade it.

All these technologies are necessary to get ready for the time when we need to use Illinois basin coal and other basins of coal, particularly in this country the tremendous amounts of bituminous coal.

Now, you notice in the testimony I offered today I realize the difficulties of budget process. And we are not saying that we should go and build pilot plants—demonstration plants, even though these—to develop the technology, I think are absolutely essential.

We are trying to work within the framework of the national need and use existing facilities to operate new technologies, new ideas a little bit past the bench scale to their process research unit, maybe a process development unit in the hopes that, once you get the momentum built up, we can establish a long-term policy that will allow us to carry on to the necessary last two links in the chain.

To do those pilot plant—demonstration plants requires much larger budgets than requested here.

Now, my last comment I want to make is: since we all talk about gas, I'd like you to take back with you the fact that utilizing coal through the first step of gasifying the coal is the most efficient and also a most environmentally acceptable way of transforming a hard-to-handle, a dirty, high-sulfur coal into a clean fuel.

Now, that fuel can be burned as high Btu gas, as Mr. Lawrence says; can be burned as medium Btu gas and, if you look at producing liquids, even, the bulk of the money goes to conversion of the coal to gas and then in either a direct or indirect liquefaction.

So gasification is a fundamental building block to the utilization of coal, via any scheme.

Mr. **Durbin**. Let me just say, in closing, I am very impressed with your testimony this morning. I think it not only points to some specifics, but it points to a general observation: that the philosophy of business and government not working together and being off on their own in separate parts of our economic system, I think, is greatly challenged by your testimony.

I think you've shown some very healthy economic approaches here, which can suggest to us that there is a need for a linkage and a partnership.

Many nations—most industrialized nations, have realized this long before us and I think it'll be a lot healthier for our economic recovery when we come to realize that business and government can cooperate for the betterment of this Nation and our overall improvement.

Thank you.

Mr. **Fuqua**. Thank you, Mr. Durbin.

Mr. Bateman.

Mr. **Bateman**. Thank you, Mr. Chairman. I, too, have certainly benefited from the testimony we've heard from the witnesses this morning. It's been a very valuable learning experience for the new members of the Committee.

I am somewhat curious to have your reactions to whether or not you believe the decision to pursue the gasification project on the Great Plains that references lignite was a wise decision, relative perhaps to having done the same kind of thing with reference to high-sulfur content coal, in terms of the resource, the quantity of the resource, the diffusion of the technology.

Was the lignite-based program more desirable—I assume someone thought it was—than the coal-based project would have been?

Dr. **Lee**. I'll let Bud discuss the policy issue. Let me tell you from an engineering standpoint. The Great Plains project is absolutely the right one to take for lignite because that's what the South Africans have worked on and they have commercial experience.

Mr. **Bateman**. This is technclogy borrowed from South Africans?

Dr. **Lee**. Yes.

And our lignite is quite similar to the brown coal in South Africa. So there is no technology, as I said earlier, that's commercially available for handling the huge amounts of bituminous, caking, high-sulfur coal. That's why we are saying we should do that.

And, as far as getting the Great Plains coal, I think it's the best thing—we should have done it years ago—to get the experience in-hand, get the true cost, get the true data from an operating plant in the United States.

Mr. **Bateman**. Would it be perhaps more accurate to say that the only existing, demonstrated technology relates to the South African lignite and that there is, at least on some confident level of predictability, a technology that applies to others that just hasn't been demonstrated? Is that a fair statement?

Dr. **Lee**. South Africa doesn't have bituminous coal. If they had, I would have predicted that there would be a technology that we could buy to do that, but today there isn't any plant of that magnitude—of that size—that's in our part of the country.

Mr. **Bateman**. My point is: is not the technology based there to do it with reference to bituminous coal, but it just hasn't been demonstrated, as has been the case with the South Africans and lignite?

Dr. **Lee**. Very true.

Mr. **Bateman**. Do other witnesses have comments they'd like to make?

Mr. **Lawrence**. Well, just very briefly, I would certainly subscribe to what Bernie has said on the technology of it. I think the policy considerations, Congressman Bateman, are that you kind of have to dance with the one that brought you.

At the same time, there were projects going on in the Four Corners area, the Indian tribal lands up in Wyoming and those projects—which had high- and low-sulfur coal as their resource base, but they got involved in environmental and land disputes and regulatory delays and the Great Plains project, with the way the State of North Dakota pursued it with them—it was kind of the survivor.

Mr. **Bateman**. Well, let me make it clear that my questions don't imply any criticism of where it's being done or with what it's being done. I'm just seeking information.

Another area of concern that I have here is: I hear your presentation as saying that there has not been requested a high enough level of Federal expenditure for research and development in the area of alternative energy sources, and that is most acutely true as to gas-oriented projects; and that, even within what has been requested, the allocation has not been an appropriate one. You make a very strong case for those points of view.

Obviously, and as you have recognized, the supply of money by the Federal Government is perhaps more limited than the supply of energy at this particular point. So we do have to wrestle with a finite supply of the money. But let us assume that, given our total priorities and the status of the economy, more money is put into research and specifically directed toward gas-related research. Industry—your organizations—are expending very substantial amounts of money in gas research.

If we threw 50 additional millions of dollars into appropriations for gas-related research, is there some institutionalized framework within which we could be assured that the additional 50 million of Federal money is going to be expended in the right place for the right projects and it's going to have some rational relationship to the kind of projects that industry is undertaking?

How do you get any coordination as to how the money would be well-spent? Because, if we were to do it, it would certainly be more than unfortunate if we extracted the money from the taxpayers, directed it to this, but found that it all got accomplished in projects that overlapped or were of less significance.

What assurances can you offer me that we'd have a coordinated research program?

Mr. **Webb**. Excuse me, Congressman. Gas Research Institute has a participation agreement with the Department of Energy. It's signed by the Under-Secretary of the Department of Energy and by the President of GRI.

In there, we have a requirement that we meet to discuss our programs and their programs. In addition to this overall broad-umbrella agreement, we have agreements with them in biomass and in unconventional gas. We also have a joint program in coal gasification going clear back to AGA in 1971. So there are a lot of formal mechanisms.

As far as the overall policy, or something, regarding additional funds, I think you ought to look at three criteria.

No. 1: many of the projects that we are recommending are to continue research that's going on right now.

No. 2 Dr. Lee is recommending we use facilities that have already been built, either by the private sector or by Federal expenditures, to do research in those facilities. I think that's a very sound policy.

Third, they ought to look at the areas we've recommended where we think there's a clear, long-term role. And I think, within those confines and working with the industry, that the money could be well-spent.

Mr. **Bateman**. Can I follow up on that? I'll do this very quickly. Is it possible to inventory those projects which under the proposed level of expenditure would have to be eliminated and to perhaps put them in order of priority as to which ones would be most missed if not carried forward?

Has anyone come up with that kind of a list or is that something that could be done?

Mr. **Webb**. Mr. Congressman, I'm sure it could be done. If you are asking: have we done that?, No, we have not.

Part of that is a prerogative of you and the committee here to ask the Department of Energy to tell you what their priorities are.

Our recommendations, I think, are consistent with the Administration's policy. I think they follow the general guidelines we've talked about this morning and we have given you, if you would, the priorities of where we think the funds ought to go and, within there, we've got some specific details of specific projects that ought to be done.

But I don't think you could just take our inputs and establish the priorities. While I would like that, I don't think that that would really be sound national planning.

Mr. **Bateman**. Thank you very much.

Mr. **Fuqua**. We thank you very much for your testimony today. We have a vote on. The committee will stand in recess for a few minutes while we run and vote, and we'll be right back.

You are dismissed.

[Recess taken.]

Mr. FUQUA. The subcommittee will be in order.

Our second panel consists of Mr. Tobias Anthony, the director of Business Development and Washington Affairs for Research, Cottrell, Inc.; Dr. Michael Massey, with Environmental Research and Technology, Inc.; and Mr. William N. Irving, vice president of Charles T. Main, Inc.

Mr. Anthony, we will be pleased to hear from you.

As I mentioned to the previous panel, if you have prepared statements and wish to submit them for the record and summarize, we would be pleased to accommodate you.

## STATEMENT OF TOBIAS ANTHONY, VICE PRESIDENT, BUSINESS DEVELOPMENT AND WASHINGTON AFFAIRS, RESEARCH-COTTRELL, INC., WASHINGTON, D.C.

Mr. ANTHONY. Mr. Chairman, I'm grateful again for this invitation. I'd like to announce that since first appearing before you in 1981 when I was director, I have become vice president, and I am sure it has to do with all the testimony that I have been providing before your subcommittee.

Mr. FUQUA. We will correct the record to reflect that you are vice president. I apologize.

Mr. ANTHONY. No apology necessary.

I would like to express my views on Federal industrial roles in energy research, development, and demonstration.

I will keep my comments brief and limit them to the subject of the R&D needed to make this Nation secure from foreign threats to our energy sources. This is going to sound very much like the speeches in the panel before us, only we are going to be talking about coal this time.

It remains clear, Mr. Chairman, that the only acceptable energy source that will guarantee that security is coal. Furthermore, the public demands that when we burn it, we must do it cleanly. That qualifies the issue of energy security as a matter of national policy, and the only body politic I know with authority to develop and execute such a policy is the Federal Government.

That means to me that the public perception of energy security rivals its perception of defense. Coupled with that perception is the given: They expect coal to be used in as benign a way as would be natural gas and oil. While the public is no longer anxious or perceives an immediate threat, I believe our mission is to prepare for the next possibility of threat. While the public no longer seems willing to conduct a crash program that would substitute coal for oil, we, nevertheless, must provide technologies that would allow for that conversion should we perceive a future threat.

This calls for a Federal mandate. The private sector is not conditioned to respond to international emergencies. But, with Federal leadership setting the course, the private sector can supply the solutions.

The administration's request for authorization does not reflect properly the respective roles of the Federal Government, which must lead, and the private sector, which must respond. A reversal of roles I believe will never again take place because we realize energy security is the province of the Federal Government.

Whatever application technology policy emerges, it must find that the public expects coal combustion to be as benign as oil combustion. For example, the public seems to be calling for acid rain control. Whether it is legislated this session, the next one, or ever will be a political or an environmental effects decision. However, it should be Federal policy to provide a cost-effective technology in the event it is called for.

I suggest that such a technology can be added to other strategies with minor modifications to existing powerplants at $60 to $70 a kilowatt, which will reduce sulfur dioxide by 50 to 60 percent and nitrogen dioxide by 30 percent. Yet, no provision has been made for such a development in the administration request.

In another matter, the administration proposes to eliminate advanced combustion technology and, therefore, further development of coal-water combustion technology. Yet, a 75–25 percent mixture appears to burn with the flame characteristics of oil.

Finally, the direct utilization budget will be emasculated from $10.6 million to $4.8 million. That is where $NO_x$ reduction work is performed which is related to control of acid emissions from coal combustion.

The private sector will not develop such technologies independent of Federal participation. The reason lies in the unpredictability of international events and, therefore, energy national policy.

Mr. Chairman, I believe it will take substantially more coal research in fiscal 1984 than the administration proposes for the Federal Government to discharge its responsibilities, but I suggest some controls are needed to avoid funding research for research's sake by the private sector.

(A) Require that DOE submit a strategy of technology development that would allow us to become coal based, energy independent in 5 to 10 years.

(B) Restore authorizations to fiscal 1983 levels of around $300 million.

(C) Urge the administration to proceed with projects up to and including small demonstrations so that we may obtain design values for commercial scaleup.

(D) Consider the principle that applied technology should be demonstrated within 4 to 5 years or be considered for cancellation.

Mr. Chairman, I believe the Federal Government and the private sector are locked into a relationship similar to the military-industrial complex. Why not? The threat we experienced in 1973, and once again in 1977, overshadowed the private sector's free enterprise role in our economy. And worse—the threat could happen again.

The quiescent prospect of a future threat thwarts any sort of rallying call, but let us be wiser now. Let us develop and demonstrate technologies as we do military weapons, to the point where they can be put into emergency production if the need arises.

Thank you, sir.

[The prepared statement of Mr. Anthony follows:]

# STATEMENT ON THE
# DEPARTMENT OF ENERGY
# FISCAL YEAR 1984 CONGRESSIONAL
# BUDGET REQUEST FOSSIL ENERGY
# RESEARCH AND DEVELOPMENT

## COAL

before the Subcommittee on Energy
Development and Application
of the
Committee on Science and Technology
U.S. House of Representatives

by

Tobias Anthony
Vice President, Research-Cottrell

March 16, 1983

Good morning Mr. Chairman and members of the Energy Development and Applications Subcommittee. Once again I feel privileged to be called before you to present my views on the federal role in energy research, development and demonstration.

Mr. Chairman, the relationship between the private sector and the federal government has changed dramatically over the past several years. Because our energy security was threatened in 1973 and once again in 1977, the federal government understandably, as in any national emergency, emerged to lead the country toward energy independence. I recall vividly President Carter's "moral-equivalent-of-war speech" six years ago next month in which he announced that independence would be based on doubling consumption of coal by 1987.

Predictably, the federal government would become the stewards of coal production, shipment and use. Furthermore, research and development programs would be accelerated to assure that coal would be used as safely and cleanly as oil. A plethora of laws were passed to meet that goal. Amendments to the Clean Air Act were passed in 1979. Other laws were passed to control surface mining, mine safety, handling of solid waste, water use and fuel use as well as to encourage production of synthetic fuels.

Gradually, the coal-related part of the private sector and Washington blended into a relationship which is beginning to resemble another security conscious sector we know as the military-industrial complex. While we have not proceeded so far, many similarities exist. Every facet of coal production, transportation or use now comes under federal policy. No longer does the private sector make investments independent of that policy. I can go so far as to say, since the synfuels express was derailed in 1981, no private firm is likely to engage in any new venture involving a long range commitment to coal, without federal leadership, or more importantly, participation.

The synfuels experience proved that the federal government is in charge of energy policy and could change that policy quickly and completely, literally overnight.

And well it should be in charge. The oil crisis was international in nature and very much the same as a military threat. Such threats require mobilization of national resources which only the federal government can muster as it would in preparation for war, albeit the moral equivalent of war. For six years now the energy-government-industry complex has drawn tighter and closer together because of laws and regulations in place. It is not likely the roles will return to the freer, enterprise relationship that existed before 1973 because the potential remains that a new international threat will surface any time in the future.

This is why I cannot agree with the Administration that the private sector will independently provide the technology needed to make us a coal-based, energy-independent nation. Furthermore, concerns that coal is not benign and that without controls it cannot be burned as cleanly as oil, assure long term federal stewardship over its use. Finally, it takes ten or more years for a firm to recover its investment in R&D. In a market subject to unpredictable factors, it is too long a period for private firms to assume exclusive risk. As a consequence, my guess is federal funding will have to cover at least half that period; the first-half, high-risk period.

How much progress have we made? Well, we cannot substitute coal for oil. Coal consumption in 1977 was 600 MMTPY and is now a little over 700 MMTPY. Almost all of that increase goes toward producing electricity in conventional ways. If we had been on Mr. Carter's track consumption would be up to 900 MMTPY. It took the government three years to start up the "synfuels express" and another three years to derail it. Meanwhile we have some low key R&D underway but the Administration requests for authorization will reduce those efforts drastically from some $500 MM in FY82 to $109 MM in FY84. Therefore, we will stop short of optimizing synthetic liquids and gas development and super clean coal.

That's not much progress.

Mr. Chairman, if the federal government withdraws from its responsibilities, there will be a void created that will not be filled by the private sector. We will simply find things to do in a market where we can exercise some leadership. And once again the nation will become vulnerable.

I suggest that at a minimum Congress authorize spending for the demonstration of technologies that will permit the substitution of coal for oil to allow it to be burned cleanly and efficiently. At least that would provide the nation with the where-with-all to go into production should a threat arise.

My qualifications limit me to speak to advanced research, control technology, coal preparation and combustion. Those programs are to be cut in half from $112 million in FY82 to $56 millin for FY84. The control technology and coal preparation category should be restored to FY82 levels at least. We need a super-clean coal absent of pollutants such that it can be burned economically in conventional utility and industrial furnaces. If we had that coal today the debate over acid rain control would be less heated. A good deal of basic research is needed if we are to have one even by the next century. In the meantime, DOE needs to fund development of an SO2 clean-up technology for existing sites where back-fit scrubbers don't fit. It would be supplemental strategy to coal washing, scrubbers or fuel switching in that SO2 reductions of 50-60% would be possible. Whether acid rain controls will be legislated or not will be a political decision. Consequently, low cost, technologies which can be installed in existing sites should be availble if needed.

We need to know more about coal-water mixtures and combustion control. Coal-water mixtures show promise of direct substitution for oil. Yet, the Administration plans to phase out all work. We need to know more about flame chemistry in direct utilization. NOx control shows great promise. But that item is being cut in half, as well.

Mr. Chairman, I could go on and on. Permit me instead to offer a few suggestions for your consideration. My purpose is to urge you to consider maintaining a high level of presence in technology development and demonstration, although at a much more modest level than in 1980.

- o DOE should prepare a strategy that would allow us to become a coal-based economy in 5-10 years.
- o Restore authorizations to FY83 levels of around $300 MM, assuming that a sound strategy will call for authorizations of $500MM or perhaps higher in FY85.
- o Urge the Administration to proceed with projects up to and including small demonstrations so that design values for commercial scale-up will be available.
- o Consider the principal that applied technology should be demonstrated within 4-5 years or be cancelled.

Finally, the staff has asked me to comment on how effective federal programs have been in advancing specific technologies. Without such funding there would be no new technologies. My guess is these technologies would be commercialized if federal policy on coal combustion and clean-up was frozen. The private sector is timid when it comes to instituting such technologies for fear a regulatory agency will consider preventing their use. Secondly, it is difficult for the users to make economic judgements because of fluctuations in the cost-of-capital, policy over fuel-price deregulation, tax policy and the economy. We have spray-driers, more efficient scrubbers and atmospheric fludized bed technologies available today because of federal funding. More would be used if energy policy would be more predictable.

Once again I am grateful to you for this opportunity. I will be happy to answer any questions you may have.

Tobias Anthony is Vice President of Business Development and Washington Affairs for Research-Cottrell Inc., a New Jersey based Fortune 500 firm specilizing in construction of pollution control facilities for power plants and in oil and gas drilling equipment. He is also the current chairman of the Environmental Industry Council.

He owns a degree in chemistry from Holy Cross College received in 1946 upon completion of an NROTC program during World War II. He received a commission in the US Navy and upon release from active duty entered Queens College to pursue graduate studies in economics and finance.

Mr. Anthony has been with Research-Cottrell for ten years. During this period he was engaged in acquisitions before moving to Washington, D.C. in 1977 to head up that office. Prior to joining Research-Cottrell, he spent eighteen years with the Grumman Corporation rising to the corporate acquisitions office before leaving in 1973.

Mr. FUQUA. Thank you, Mr. Anthony.

Dr. Massey, we will be pleased to hear from you.

## STATEMENT OF DR. MICHAEL J. MASSEY, MANAGER, ENGINEERING TECHNOLOGY DEVELOPMENT FOR ENVIRONMENTAL RESEARCH & TECHNOLOGY, INC., PITTSBURGH, PA.

Dr. MASSEY. Thank you, Mr. Chairman.

My name is Michael J. Massey. I presently serve as the manager of Engineering Technology Development for a company called Environmental Research & Technology, Inc. It is a leading full-service environmental company.

Prior to joining ERT in 1978, I served as a professor of chemical engineering and engineering and public policy at Carnegie Mellon University. I have been actively involved in coal-related research and development for over 10 years. During that time I have conducted independent chemical engineering research in both process and environmental aspects of coal gasification and coal liquefaction processing.

As a major adviser to fossil energy's program for environmental research in coal-conversion processing since 1975, I am directly familiar with the history, development, and accomplishments of the fossil energy program. It is from the perspective of this cumulative technical background that I offer the following testimony regarding the administration's proposed fiscal year 1984 budget authorization for Fossil Energy R&D.

While overlap is somewhat inescapable, I will try to direct my remarks mainly to the environmental engineering, rather than process engineering, aspects of the administration's proposed program. Further, I will focus primarily on the synthetic fuels portion of the Fossil Energy program, where my experience is greatest. In this context, my testimony will address:

One, the effects of a decontrolled energy market on private sector R&D;

Two, the Federal role in energy research, development, and demonstration; and

Three, the extent to which the private sector can be expected to pursue Government-sponsored technology development activities slated for reduction or termination.

While timeframes for reliable projection of free energy prices are necessarily short, the most likely scenario is several years of relatively stable energy prices. Despite the attractiveness of a quiet period of stable prices during which to proceed with basic research, and in spite of a distinct rise in private sector R&D over the period of the present recession, there has already been a severe downturn in near-term private sector R&D work in synthetic fuels. Without significant rises in energy prices, I see limited incentives in the foreseeable future for any new major initiatives in private sector R&D on synthetic fuels, whether of a process or an environmental nature. Under the circumstances, federally stimulated programs can be expected to constitute a major portion of total national R&D efforts on synthetic fuels in the near-term future.

Coupled with an appropriate stimulation of private sector commercialization activities, I support the administration's basic philosophy regarding the Government's role in energy R&D as outlined in the proposed fiscal year 1984 budget for Fossil Energy.

I would paraphrase that philosophy as follows:

Primary public moneys should be invested in basic science and in the early stages of technology development, where payoffs on research are frequently difficult to predict, highly risky, and/or very slow to mature. Substantial private sector funds should be invested at the later, more costly stages of process development demonstration and commercialization.

Consistent with this philosophy, the administration has shifted Federal responsibilities for the stimulation of synthetic fuel commercialization from Fossil Energy to the Synthetic Fuels Corporation and has focused Fossil Energy's efforts on long-range, high-risk R&D.

While supportive of a long-range, high-risk R&D mission for Fossil Energy, I take strong exception with the administration's assessment of the level of funding required to establish and sustain a comprehensive R&D program capable of meeting the administration's stated aims. I believe that if reduced from its present fiscal year 1983 authorization of $372 million to the administration's proposed fiscal year 1984 budget of $138 million, Fossil Energy will be forced to operate substantially below the critical mass necessary for comprehensive, long-range R&D programs capable of meeting the critical contributions called for by the administration.

An end to recent serious discrepancies between initial proposals and final budget authorizations, combined with sustained funding at the level of at least $250 to $300 million a year, would be required to achieve a quality Fossil Energy R&D program.

With limited exceptions, I do not believe that the private sector would continue the environmental R&D projects in synthetic fuels which would be dropped from the Federal program if the presently proposed administration budget was approved and sustained in future years. In today's energy market, much of Fossil Energy's near-term environmental R&D meets the administration's test of high-risk, high-payoff research which is either of the nature of primary scientific information or engineering concepts which have not yet passed the proof-of-concept phase.

I will provide at least one example shortly.

In my view, Fossil Energy's environmental R&D activities in synthetic fuels are in a distinct and critical state of transition, the substance of which must be understood to appreciate the full impact of proposed administration budget reductions.

Since its primary inception in 1975, the environmental R&D program in synthetic fuels has had three components: One, characterization of processes and control systems; two, innovative control technology development; and, three, the development of innovative controls through process optimization.

Responding first to the fundamental lack of data program activities for fiscal year 1976 through 1981, focus was heavily on pilot scale characterization of the health, environmental, and ecological properties of various synthetic fuel processes and state-of-the-art control systems. It has only been with the results of its major characterization efforts over 5 years that Fossil Energy has established a concrete basis for targeting strategic expansions of its base programs in innovative control technology development and innovative control through process optimization.

Fiscal year 1982 and fiscal year 1983 have represented years of evolution toward this heightened focus on innovative technology development. However, adequate and sustained funding over the next 5 years is required to reap the full benefits of Fossil Energy's work today.

Appropriately, Fossil Energy's shift from characterization has been accelerated by the administration's shift of primary Federal responsibilities for synthetic fuel commercialization from Fossil Energy to the Synthetic Fuels Corporation.

Fossil Energy's role in the continued health and environmental characterization of existing processes and control system has shifted from one of primary responsibility to that of largely critiquing and monitoring of private sector activities in conjunction with SFC supported projects.

Nowhere are the significance of Fossil Energy's past characterization efforts and the resultant near-term potentials for innovative technology development more evident than in synthetic fuels health effect management. Through the extensive cooperation and participation of Fossil Energy in the basic research efforts of DOE's Office of Health and Environmental Research and as a result of a strategic amount of health effects characterization work of its own, Fossil Energy has been able to establish concrete testing protocols and definitive relationships between the chemistry of various synthetic fuel liquids and their mutagenic and carcinogenic properties.

Armed with these results, initial scoping studies initiated by Fossil Energy during fiscal year 1982 and 1983 are demonstrating that those chemical constituents in synfuel liquids which are responsible for biological activity are amenable to conversion through process management and/or innovative control treatment.

What is needed now are systematic investigations of the underlying science, the strategic significance, and experimental verifications of formation and decomposition mechanisms for biologically active components. This work should proceed both on specific synthetic fuel processes and on a series of innovative techniques for raw coal liquid upgrading. The highly achievable goal is a cost-effective reduction/elimination of synthetic fuel toxicological properties through optimized processing and targeted raw product upgrading.

A similar pattern of opportunity for a combination of improved levels of control and reduced costs now exists in many other aspects of health and environmental management. However, programs to exploit these opportunities are seriously threatened by budgets of the level presently proposed by the administration. If such budgets are approved and sustained for a period of years, Fossil Energy will be forced to reduce the number of projects, the scale of experimental studies, and the number of technical teams involved in high risk, high-payoff synthetic fuels R&D. Early evidence of such ominous trends is already apparent in the administration's proposed budget for fiscal year 1984.

For example, that budget makes frequent references to shifts away from most process development unit and pilot scale experimental efforts in favor of much lower cost theoretical work and benchscale experimentation. While larger scale test systems are not entirely eliminated in the administration's proposed fiscal year 1984 budget, sustained budgets at this level would inevitably have that effect. Given the complex nature of coal processing and the challenging issues of engineering scaleup, I seriously question whether the administration's goal of proof-of-concept programs can be achieved in most cases without at least process development unit-scale testing.

In summary, I believe that the past budget reductions in Fossil Energy have been justified in view of the shift away from commercialization and toward long-range, high-risk R&D. However, presently proposed budgets threaten to undermine Fossil Energy's basic capability to capitalize effectively on the substantial base of information and strategic sense of direction which it has developed over a period of 5years.

Under present and foreseeable circumstances, I do not anticipate that the private sector will proceed in a timely manner with most of the long-range, high-risk R&D which would be dropped from the Federal program.

Thank you, Mr. Chairman.

Mr. **FUQUA**. Thank you very much, Dr. Massey.

[The prepared statement of Dr. Massey follows:]

## TESTIMONY ON FISCAL YEAR 1984 AUTHORIZATION FOR THE DEVELOPMENT OF ENERGY'S FOSSIL ENERGY RESEARCH AND DEVELOPMENT PROGRAM

(By Dr. Michael J. Massey)

My name is Michael J. Massey. I presently serve as Manager of Engineering Technology Development for Environmental Research & Technology, Inc. (ERT) a leading full service environmental company. Prior to joining ERT in 1978, I served as a professor of Chemical Engineering and Engineering and Public Policy at Carnegie Mellon University. I have been actively involved in coal-related research and development for over 10 years. During that time, I have conducted independent chemical engineering research in both process and environmental aspects of coal gasification and coal liquefaction processing. As a major adviser to Fossil Energy's program for environmental research in coal-conversion processing since 1975, I am directly familiar with the history, development and accomplishments of the Fossil Energy program. It is from the perspective of this cumulative technical background that I offer the following testimony regarding the Administration's proposed fiscal year 1984 budget authorization for Fossil Energy R&D.

While overlay is inescapable, I will try to direct my remarks mainly to environmental engineering rather than process engineering aspects of the Administration's proposed program. Further, I will focus primarily on the synthetic fuels portion of the Fossil Energy program, where my experience is greatest. In this context, my testimony will address:

(1) The effects of a decontrolled market on private sector R&D;

(2) The federal role in energy research, development and demonstration; and

(3) the extent to which the private sector can be expected to pursue government-sponsored technology development activities slated for reduction or termination.

While timeframes for reliable projection of free energy prices are necessarily short, I presently see at least several years of relatively stable energy prices. Despite the attractiveness of a quiet period of stable prices during which to proceed with basic research, and in spite of a distinct rise in private sector R&D overall during the present recession period, there has already been a severe downturn in near-term private sector R&D work in synthetic fuels. Without significant rises in energy prices, I see limited incentives in the foreseeable future for any new major initiatives in private sector R&D on synthetic fuels, whether of a process or an environmental nature. Under the circumstances, federally stimulated programs can be expected to constitute a major portion of total national R&D efforts on synthetic fuels in the near-term future.

## PROPER FEDERAL ROLE IN R&D

Coupled with an appropriate stimulation of private sector commercialization activities, I support the Administration's basic philosophy regarding government's role in energy R&D as outlined in the proposed FY 1984 budget for Fossil Energy:

"Systematic technological advances must occur in all stages of the research, development, and commercialization cycle—from basic science to applied research, proof-of-concept, process development, and finally to commercialization. The latter stages of this cycle are the most costly, and this is where extensive private sector investments will be required to take advanced technologies into commercial reality. The earlier stages, through proof-of-concept, feed this research and development investment pipeline through the results of long term, high risk and potentially high payoff research. While R&D in these earlier stages may have a very high benefit/cost ratio from the public perspective, private sector investment may be restricted or non-existent because payoffs are frequently difficult to predict, highly risky, or long term, or cannot be captured by private firms." (Pg. 15 of FE Budget Request for fiscal year 1984).

Consistent with this philosophy, the Administration has shifted federal responsibilities for the stimulation of synthetic fuel commercialization activities from Fossil Energy to the Synthetic Fuels Corporation and has focused Fossil Energy's efforts on long-range, high-risk R&D. While supportive of a long range, high risk R&D mission for Fossil Energy, I take strong exception to the Administration's assessment of the level of funding required to establish and sustain a comprehensive R&D program capable of meeting the Administration's stated aims, viz.,

I believe that, at the Administration's proposed fiscal year 1984 budget of $138 million, Fossil Energy would be forced to operate substantially below the critical mass necessary for comprehensive long term R&D programs capable of making the critical contributions called for by the Administration. An end to recent serious discrepancies between initial proposals and final budget authorizations combined with sustained funding at the level of at least $250 to $300 million a year would be required to achieve a quality Fossil Energy R&D program.

With limited exceptions, I do not believe that the private sector would continue the environmental R&D projects in synthetic fuels which would be dropped from the federal program if the presently proposed Administration budget was approved and sustained in future years. In today's energy market, much of Fossil Energy's near term environmental R&D meets the Administration's test of high risk, high payoff research which is either of the nature of primary scientific information or engineering concepts which have not yet passed the proof-of-concept phase.

In my view, Fossil Energy's environmental R&D activities in synthetic fuels are in a distinct and critical state of transition, the substance of which must be understood to appreciate the full impact of proposed Administration budget reductions. Since its primary inception in 1975, the environmental R&D program in synthetic fuels has had three components, (1) characterization of processes and control systems, (2) innovative control technology development, and (3) the development of innovative controls through process optimization. Responding to the severe lack of initial data, a major focus of programs from fiscal year 1976 through fiscal year 1981 was on component 1, viz., pilot scale characterization of the health, environmental, and ecological properties of various synthetic fuel processes and state-of-the-art control systems. It has only been with the results of its major characterization efforts over five years that Fossil Energy has for targeting the strategic growth of its basic programs in innovative control technology development and innovative control through process optimization. Fiscal year 1982 and fiscal year 1983 have represented years of evolution toward this heightened focus on innovative technology development. This has been accelerated by the Administration's shift of primary federal responsibilities for synfuels commercialization from Fossil Energy to the Synthetic Fuels Corporation. Fossil Energy's role in the continued health and environmental characterization of existing processes and control systems has shifted from one of primary responsibility to that of largely critiquing and monitoring of private sector activities in conjunction with SFC—supported projects.[1]

Nowhere are the significance of Fossil's Energy's past characterization efforts and the resultant near term potentials for innovative technology development more evident than in synfuels health management. Through extensive cooperation and participation with the basic research efforts of DOE's Office of Health and Environmental Research (OHER) and a strategic amount of health effects characterization work of its own, Fossil Energy has been able to establish concrete testing protocols and definitive relationships between the chemistry of various synfuel liquids and their mutagenic and carcinogenic properties. Armed with these results, initial scoping studies initiated by Fossil Energy during fiscal year 1982 and fiscal year 1983 are demonstrating that those chemical constituents in synfuel liquids which are responsible for biological activity are amendable to conversion through process management and/or innovative control treatment. What is needed now are systematic investigations of the underlying science, the strategic significance, and experimental verifications of formation and decomposition mechanisms for biologically active components. This work should proceed both on specific synthetic fuel processes and on a series of innovative techniques for raw liquids upgrading. The highly achievable goal is a cost effective reduction/elimination of synfuel toxicological properties through optimized processing and targeted raw product upgrading.

A similar pattern of opportunity for a combihation of improved levels of control and reduced costs now exists in many other aspects of health and environmental management. However, programs to exploit these opportunities are seriously threatened by budgets of the level presently proposed by the Administration. If such budgets are approved and sustained for a period of years, fossil energy will be forced to reduce the number of projects, the scale of experimental studies, and the number of technical teams involved in high-risk, high-payoff synthetic fuels R&D. Early evidence of such onimous trends is already apparent in the Administration's proposed budget for fiscal year 1984. For example, that budget makes frequent references to shifts away from most process development unit and pilot scale experimental efforts in favor of much lower cost theoretical work and benchscale experimentation. While larger scale test systems are not entirely eliminated in the Administration's proposed fiscal year 1984 budget, sustained budgets at this level would inevitably have that effect. Given the complex nature of coal processing and the challenging issues of engineering scaleup, I seriously question whether the Administration's goal of proof-of-concept programs can be achieved in most cases without at least process development unit-scale testing.

In summary, I believe that past budget reductions in fossil energy have been justified in view of the shift away from commercialization and toward long-range, high-risk R&D. However, presently proposed budgets threaten to undermine fossil energy's basic capability for timely comprehensive contributions to cost effective, environmentally acceptable utilization of the nation's domestic energy resources. Under present and foreseeable circumstances, I do not anticipate that the private sector will proceed in a timely manner with most of long range, high risk R&D which would be dropped from the Federal program.

Mr. FUQUA. Thank you very much, Dr. Massey.

Mr. Irving, we would be pleased to hear from you at this time.

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[1] Indeed, the evolution of characterization activities from federally funded high-risk programs on pilot scale systems to private sector-funded characterization activities on commercial-scale facilities represents a major successful outcome in the administration's model for proper federal involvement.

## STATEMENT OF WILLIAM N. IRVING, DIRECTOR AND ADVISORY BOARD MEMBER, MHD INDUSTRIAL FORUM, WASHINGTON, D.C.

Mr. IRVING. Mr. Chairman, thank you very much for the opportunity to appear here before you.

My name is William Irving. I'm director and advisory board member of the MHD Industrial Forum. I am also a vice president of Charles T. Main, Inc., an international architectural engineer located in Boston, Mass.

I have with me today Mr. Vincent Coates, vice president and director of the MHD Industrial Forum and director of special projects at AVCO Everett, and Dr. Susan Wu, a member of the advisory board of the forum and administrator of energy conversion research and development and professor of aerospace engineering, University of Tennessee Space Institute. Also behind me is Mr. Jack Noble, chief mechanical engineer of Charles T. Main and very experienced in the technology of magnetohydrodynamics.

We appreciate your asking us to submit the MHD Forum's document entitled "The Early Commercial Development of MHD for the Generation of Electric Power," to the committee and for the opportunity to testify. I ask that the strategy document be made a part of the record.

Mr. FUQUA. We will make them part of the committee files on the hearing.

Mr. IRVING. Thank you.

To refresh some of you who may not be that close to the technology of MHD, MHD is a method of generating electric power using coal as fuel, though not limited to coal, which heats a gas stream which is then electrified or ionized by potassium seed. This stream passes through a magnetic field, and direct current power is taken directly from the channel through which the stream passes and then is converted to alternating current and introduced to the electric utility grid. It is simple in concept. It works and is proven. It has no moving parts in the main part of the cycle.

Its key advantages are improved efficiency and the kindness to our environment. The efficiency is extremely important. I would like to take just a moment to explain that to the committee.

Our usual fossil energy plants as operating today have an average of 37 percent efficiency. In other words, 37 percent of the heat value in the fuel is used to generate electricity. We are, thus, throwing away, wasting, discharging to the environment two-thirds essentially of the heat in the fuel. This is necessary inherently because of the steam cycles that are used, be it in coal, oil, or gas-fired cycles, nuclear.

MHD attacks this problem in a different way and is not limited by the steam cycle. Thus, we project early efficiencies of 45 percent of the total cycle, increasing as time goes on to 50 percent in the late nineties, and with no limit up to perhaps 65 percent in the distant future.

What does this mean to you, me, and our people? If I take the installed capacity necessary for, let's say, the city of Boston, 2,000 megawatts, and I operate with current equipment using 37 percent efficiency, then compare it with MHD, a very attainable goal in my opinion in the moderate-term future of 50 percent, we will save 1 million tons of coal per year, which translates to $50 million per year.

In my strong opinion, this is worth looking very carefully at as far as evaluation of where our dollars go for research.

I indicated MHD is not new. Development in the United States has progressed to a point where many of the important components are developed and are ready for full-scale demonstration. The basic research is essentially complete. We now need demonstration on a utility system.

To achieve commercially available plants in the nineties, we need, No. 1, to focus on the essential technology leading to an MHD plant on the electric utility site as soon as possible. We need a financing plan involving the utilities, private industry, and Government. We need continuing R&D to support commercialization and long-term improvements. We need a well-defined schedule. We need a multiyear congressional authorization for funding. We strongly recommend a minimum of $40 million for fiscal year 1984 from the Federal Government.

In response to the need and encouragement of many congressional Members, the MHD Industrial Forum, prepared the strategy that I mentioned earlier. It was prepared by the forum and leaders in the MHD field, electric utilities, manufacturers and suppliers, R&D firms, and colleges and universities.

It is a broad assessment of the requirements to design, construct, and operate on a utility system as soon as possible, which will be a major step toward commercial MHD units.

Our proposal here is different from previous proposals which involve totally new MHD powerplants. Our proposal centers on a retrofit of an existing fossil fuel-fired plant. This retrofit could increase the capacity of that plant by about 50 percent.

Our strategy includes operation by 1991–92 of approximately 120 megawatts of capacity, of which 50 megawatts is MHD. This size is adequate for demonstrating reliable and operations and obtaining environmental data for commercial evaluation at a cost far less than a full-sized demonstration and with more testing and operating flexibility.

It also reduces environmental impact statement efforts and permit requirements.

We are not promising that this first retrofit is going to be a fully successful commercial method of generating power. We are saying that it may very well turn out to be that, and it is the shortest route to commercial magnetohydrodynamics.

We would propose a consortium for such a program made up as follows: A utility for the site, program management, furnish operating personnel, and to solicit other utilities to assist in the funding of commercially available components; private industry for component design and fabrication and engineering services at zero or minimum profit; the Federal Government to continue funding the MHD program and furnish components resulting from this program; and, lastly, State and local governments for licensing and environmental studies, to minimize the delay and costs.

We estimate that in 1983 dollars the cost of the program we propose will be $325 million, and our schedule is as follows:

In 1983 or 1984, select a site and make the financial arrangements for the program.

In 1985, start the detailed engineering and component procurement.

In 1986, we would start construction.

In 1988, the MHD segment of the plan would be ready for test operation.

In 1988 and 1989, make necessary modifications, which most certainly will come out in this first demonstration.

Late 1989, initial operation of the entire combined plant.

1991 and beyond, operation by the utility of this combined MHD steam plant.

Now the retrofit approach does not eliminate research and development. Research and development will continue for some time but we foresee the necessity for Federal Government support falling off in the early nineties, if things go as we predict they will.

MHD, like any advanced technology, will require improvements in availability, reliability, and efficiency, and yes, on environmental impact.

The forum believes strongly that private funds will result when industry sees Government committed to a well-defined program and a clearly defined time schedule. Several utilities have investigated and studied MHD retrofits. The Montana Power Co. just recently submitted a proposal to DOE to cost share in an MHD program.

We have an MHD users committee under the direction of Dr. Heinz Pfieffer of Pennsylvania Power & Light, and this users committee is comprised of key people from virtually all large coal-fired electric utilities in the country. The enthusiasm and participation has been very impressive. Dr. Pfieffer helped us in the preparation of the report we have mentioned.

Truly no advanced enegy technology will achieve commercial use without Government support under our system.

We are recommending, yes, an ambitious program but compare for a moment: in Russia they have a 25-megawatt unit in service. I have seen that in operation. They are now designing a 500-megawatt MHD plant in Russia. It is going on. It is successful. In Russia, however, they are not concentrating on coal. They have an abundance of gas but they will be going to coal in the future.

Also, MHD as it's developed today was helped considerably by the space program, the fallout of high temperature material research, and, in turn, development of MHD already has and certainly will further result in similar fallout for future developments. Examples are high temperature materials, diagnostics instrumentation as applied to current-type plants, and pollution control for conventional powerplants.

It is imperative that the utility be a key in any such program, as we are recommending. This is the way we will get it done. This will interest other utilities, solicit additional private funding, and, therefore, the utility that is chosen for this host demonstration must play a very, very key role.

The forum hopes that the strategy we have presented here to you and in our booklet will be helpful to the committee and will result in legislation authorizing a program leading to the operation of MHD on a utility site in the early nineties.

The well-being of our citizens in the future will depend heavily on the availability of reliable electric power at the lowest possible cost and with minimum impact on the environment. In this competitive world today low cost power is a critical factor in maintaining and improving our position industrially and for the well-being of our people.

MHD, given the opportunity and with the extremely encouraging progress to date to the work in the past, will play an important part in attaining this objective.

Thank you, sir.

[The prepared statement of Mr. Irving follows:]

Statement of

Mr. William N. Irving
Director and Advisory Board Member
MHD Industrial Forum
1725 DeSales Street, NW
Washington, DC 20036

for

The Subcommittee on Energy Development and Applications
of
The Committee on Science and Technology
U.S. House of Representatives

16 March 1983

## SUMMARY

There is a growing concensus within the electric utility industry, its industrial suppliers, the MHD research and development community and some segments of the Federal Government that the introduction of MHD electric power generation into a utility use within a decade is a realistic goal. This results from the important advances that have been made in MHD technology over the past several years, particularly in components, and the continued confirmation of the MHD's economic and environmental advantages.

It also is realized that to accomplish this goal will require certain commitments. 1) An effort that focuses on essential technology tasks leading to an MHD plant that will operate on a utility site at the earliest possible time; 2) A financing plan that involves utilities, private industry and the government; 3) A continuing research and development effort not only to support the commercialization goal but also for long-term improvements; 4) a well-defined time frame in which to accomplish all this; 5) a multi-year Congressional authorization; and 6) a minimum of $40 million for fiscal 1984.

In response to this concensus and at the encouragement of many in the Congress, the MHD Industrial Forum has prepared a "Strategy for the Early Commercial Development of MHD for the Generation of Electric Power."

The Strategy was prepared by a committee of the MHD Industrial Forum consisting of leaders in the field of MHD representing the electric utility industry, utility manufacturers and suppliers, industrial research firms, and universities and colleges.

We wish to emphasize at the outset that this Strategy is a broad assessment of what would be required to design, construct and operate a MHD power generator on a utility site at the earliest possible time. With success this recommended plant can be the major step as the precursor to future commercially viable units.

Unlike previous proposals which called for a completely new power plant to be operational in the late 1990's, this Strategy centers around a "retrofit" e.g., the addition of an MHD unit to an existing coal burning power plant. Since an MHD plant includes a conventional steam turbine, the retrofit MHD plant will utilize the existing turbine generator, cooling system, support functions and personnel on an operating site. In addition to being significantly less costly than a completely new plant, the retrofit will result in about a 50 percent increase in power generation capability.

The retrofitted plant would be operational by 1992 with a nominal output of 120-130 megawatts of which 50 megawatts would be obtained from the MHD cycle. This size is optimum to provide all the reliability and operational data required by the utility industry for final commercial evaluation and at a cost far less than an full-sized demonstration plant but with more testing and operational flexibility. This option also reduces environmental impact studies and permit requirements.

The initial retrofit may not actually be commercially viable but would provide the basis for commercial viability in the shortest practical time span through a consortium effort involving utilities, industry and the government. One consortium possibility is:

- a utility to provide the retrofit site, program management, and operating personnel, with other utilities assisting the funding of commercially available components;

- private industry to provide component design, and fabrication, and engineering services at minimum profit;

- the Federal Government to continue funding the MHD program and to furnish components resulting from this program;

- the State/Local Governments to assist in licensing and environmental studies to minimize delay and cost.

The total cost of the retrofit is estimated at $325 million. The initial phase of the program will explore various funding possibilities for the participants. One funding possibility is for the Government to furnish those components which will come from the MHD development program and for the private sector to furnish those components which are commercially available.

The Strategy suggests selecting a retrofit site while making financial arrangements in 1983 and 1984. Detailed engineering would start early in 1985 followed by the procurement of major components, and construction would start by 1986. Since the retrofit is the first of a kind, four years are allowed for construction. The MHD segment would be ready for test operation in early 1988, allowing most of 1988 and 1989 for modifications that may be necessary prior to retrofit initial operation in late 1989 with expected utility operation by the end of 1990.

It should be understood that the retrofit approach does not eliminate the need for research and development. Indeed, to achieve its maximum promise, MHD like any other advanced technology, will require a continuing research and development program well beyond its initial commercial operation.

It is the opinion of the Forum that private funds will be forthcoming when the industry sees that the government is committed to a well-defined program within a clearly-defined time schedule. Several utility companies are already investigating possible MHD retrofit possibilities. The Montana Power Company earlier submitted a proposal to DOE to cost share in an MHD program. On the other hand, since the Government long ago assumed primary responsibility for high technology research and development, neither MHD nor any other advanced energy technology will achieve commercial use without Government support and leadership.

Finally, we recognize that we are recommending an ambitious program. However, it is a far less ambitious program than that being followed by the Soviets who are now designing a 500 MW MHD plant. The schedule is consistent with the progress of the technology development. In the absence of a reasonable target date for operating MHD on a utility site, the engineering community and the utility industry will lose interest.

We hope that this Strategy plan will be useful in the committee and will result in legislation authorizing a program that will lead to the operation of MHD on a utility site early in the next decade.

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WILLIAM M. IRVING
Director and Advisory Board Member
MHD Industrial Forum

Mr. Irving is vice president and manager of the Thermal Power Division of Chas. T. Main Inc., an international architectural and engineering firm located in Boston, Massachusetts.

A former vice president and director of research of Boston Edison Company and former chief executive officer of Jacksonville (Florida) Electric Authority, Mr. Irving serves on numerous committees for the Electric Power Research Institute (EPRI) the Edison Electric Institute (EEI) and the Electric Research Council.

He was a member of the first Avco/Electric Utility Steering Committee which directed and funded the early MHD research and development program, and subsequently served as chairman of the EEI/Avco/EPRI Steering Committee that funded and directed MHD Research and development during the 1960's and early 1970's. Mr. Irving is a professional engineer registered in Massachusetts.

The MHD Industrial Forum was established in September 1981 to focus government and public attention on the tremendous energy and environmental benefits of MHD technology. The Forum represents a broad spectrum of interests, including industrial suppliers, research and development companies, individual scientists and engineers, and utilities eager to put the technology to practical commercial use.

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Mr. **Fuqua**. Thank you, Mr. Irving.

Mr. Irving, as you are aware, the ERAB panel recommended that funding be eliminated for MHD. How do you respond to that report?

Mr. **Irving**. Mr. Chairman, I am not an advocate of any one future method of generating power. We must keep our options open. There are many viable methods of generating power that are coming to the front that are being funded by the Federal Government.

I believe that MHD has the most promise of returning dollar for research dollar of any technology in the fossil area of which I am aware. Thus, I strongly believe it would be short-sighted not to continue with MHD.

I liken this to perhaps a personal situation. If I were out of work and my assets were draining, the last thing I would let go would be my life insurance policy because it protects my people, my family, in the future. I am not saying MHD is a life-or-death situation. I am only illustrating that we must, even in tight times, put our dollars where they will do the most good for the future. If we forget about our future and just take care of the present, we will all be the losers.

Mr. **Fuqua**. Still, how do you respond to the ERAB report? They are an independent group that looked at all of the technologies, and they chose not to recommend MHD funding. I agree with you that we shouldn't put everything in one basket, but how would you respond to their report that it should be eliminated?

Mr. **Irving**. I have not read their report, sir. I do remember the ECAS studies several years back, which were very strong for MHD. I think oftentimes it is the inputs for a particular study that have an effect on the results.

Could somebody else help me here?

Mr. **Coates**. I think the Government spent, as I recall, several million dollars on the ECAS study, and review of the ECAS study shows that of all the various technologies which they reviewed, they found that MHD came out first in terms of efficiency and in terms of environmental impact. Since that study has been done, all of the advances in MHD have been very positive. We now know at AVCO that we can build a channel that will operate for 8,000 hours. With limited amounts of money, TRW has produced a combuster which we have operated with a channel. The very first tests were very satisfactory. Because of lack of money, we have not been able to continue those tests.

There is no question but we can operate an MHD channel, combuster, which are the two critical components. Superconducting magnet technology is well in hand, as you probably know from other programs.

The area that requires some additional work is heat and seed recovery, but we can operate an MHD plant without that particular end of it, at least in the early stages.

From the engineering-technical point of view, there are actually no reasons not to continue. The people who put this study together are people who have had a lot of experience and not just in MHD, and they are convinced—and they have convinced me—that it can be done by the early 1980's.

The critical question has to do, really, with which of these technologies should go forward. I have to agree with Bill that they all should go forward.

I think that Mr. Lee summed it up very well earlier. You cannot divorce defense from energy.

Dr. WU. Mr. Chairman, can I add one point here, too?

The work we are doing at the University of Tennessee is primarily concerned with the downstream portion of MHD, which we feel is extremely important in terms of efficiency. Also, we have demonstrated extremely successful environmental control techniques for control of both $NO_x$ and $SO_x$. One can virtually eliminate all the sulfur dioxide by virtue of the process itself. The MHD process itself has a bulletin environmental control mechanism because of use of seed which takes out all this sulfur. Therefore, we feel that there is really no reason not to move forward in MHD because MHD is the only high-technology R&D area in the fossil energy which offers the advantage of high efficiency and low environmental intrusion.

As far as the ERAB report, which I read a year ago, I do not know how the conclusions were drawn. I played no part in that. No one asked my opinion, nor have several other people been asked in the MHD community—none of them seems to have made contributions. Therefore, I really cannot answer you why such a recommendation was made.

From the technology point of view as well as from an economic and efficiency point of view, as Mr. Irving pointed out, AVCO, TRW, and the University of Tennessee and many other places—we see really no reason in front of us that that technology should not be pursued.

Again, I think we must have a balanced energy strategy in nuclear as well as fossil. In fossil areas, certainly coal, gas, and liquid should be pursued simultaneously.

Thank you.

Mr. FUQUA. Mr. Irving, you proposed a program of about 8 years duration, starting in 1983 and going through about 1991, leading to an operational unit through a retrofitting of a presently coal-fired plant.

What is the comparative incremental cost of the MHD equipment compared to the capital costs per KW of a coal-fired generation plant?

Mr. IRVING. The cost is about $325 million for essentially 50,000 kilowatts of electricity.

Mr. FUQUA. For how many?

Mr. IRVING. Fifty megawatts or 50,000 kilowatts.

Mr. FUQUA. OK.

Mr. IRVING. That is——

Mr. FUQUA. About $7,000 per kilowatt?

Mr. IRVING. Near $6,000 per kilowatt, which far exceeds, of course, today's cost on fossil plants which will run $1,500 to $1,700 per kilowatt. Remember, this is a demonstration. This is a learning curve. Our projected costs are that MHD fossil plants combined will be essentially the same in capital cost as the fossil plants alone.[1]

Mr. **Fuqua**. Is there anything in the utility rate structure where they now can pass through the fuel cost to the ratepayer that acts as a detriment to the development of MHD?

Mr. **Irving**. Yes, in itself it is. There are in many public service commissions in various States incentives to increase efficiency. I believe this is a good thing. Such efforts would be helpful to MHD or other more efficient generating processes. However, just passing on the fuel costs—and having been in the utility business itself most of my life, we do not look at things that way. We are trying to get the best, most efficient, and lowest cost we can. On the other hand, there is not a direct economic incentive to get better, more efficient plants.

Mr. **Fuqua**. Mr. Anthony, do you believe that DOE R&D expenditures drive up or inhibit private R&D funds?

Mr. **Anthony**. Yes, sir. I feel that DOE expenditures substitute for private investment. So I would say they inhibit private investment.

The private sector will have a difficult time replacing those kinds of dollars for long-term research or long-term applied technology.

As I noted in my testimony, I think that there are a couple of areas where the Federal responsibility is clear, and one of those areas is acid rain control. If there is an acid rain control bill that will come through the Congress, there will be many sites, utility sites, where scrubbers will not fit; there will be decisions made that low-sulfur coal will not be imported into a State; and coalwashing, while helpful, will not approach levels of 50 percent reduction. It will be a Federal policy that the private sector in will be very reluctant to try to satisfy.

Consequently, I would say DOE funds inhibit investment by private sources because energy policy is uncertain.

Mr. **Fuqua**. Well, there is a contingent that believes that the private sector fossil energy has always made the necessary R&D effort required to have an adequate fossil fuel supply. Do you agree with that, or should there be additional Federal efforts?

Mr. **Anthony**. Do you mean that the private sector has done this? No, I don't agree that the private sector has done this over the past 4 or 5 years. Prior to 1975–76, the private sector was essentially responsible for the developing technology. My company used to spend only its own money on cleanup technology, and it was not until the Environmental Protection Agency became a competitor to the pollution control industry in developing technology that we felt foolish not to participate in their programs. Since that time period, the mid-1970's, no, I do not think the private sector has supplied the long-term technology that is needed; short term, yes.

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[1] I would like to clarify these costs, by pointing out that the retrofit cost includes the majority of the cost for a plant of about 130,000 kilowatts. A more realistic comparison would indicate a cost of the retrofit (crediting the existing turbine-generator) of about $3,000 per kilowatt. Also the $325 million estimate for the retrofit considers the high cost for the first unit. Subsequent or larger plants would be less costly.

Mr. **Fuqua**. Dr. Massey, how can DOE do a better job in research? What needs to be done to do a better job in environmental research relating to the sources we are talking about?

Dr. **Massey**. I am not sure, other than evolving in the direction they are going, that they need to do anything different. I think the concern I expressed was that they are not going to be able to accomplish in the next 5 years what they would purport to do if the budgets go down the way they are proposed to do.

There is a real issue here as to how many dollars of total program are required on the process engineering level in order to create the proper opportunity for environmental work. As I listened carefully to Bernie this morning, and we have been around this many times for years between us, there is a crucial need for certain scales of experimentation in order to produce materials that have any relevance on the environmental issue level. I seriously would question whether benchscale work accomplishes that.

The sense I get from the administration's proposals is that they are going in the direction of benchscale work, and I think that is going to create some real problems.

Mr. **Fuqua**. You mentioned in your testimony, Dr. Massey, at the bottom of page 6 and top of page 7, and I will just paraphrase it, about the complex nature of coal processing and the engineering scaleup issues. You question whether the administration's goal of proof-of-concept programs can be achieved in most cases without at least a process development unit scale.

Could you expand on that? Do you know any examples of that?

Dr. **Massey**. Let me think of the latter one for a moment. The concept I think is fairly well supported by things that have happened over the past 5 years.

Let's clarify what we mean by benchscale versus PDU. Bernie made an attempt at that a little earlier.

Units that people in the engineering world consider to be benchscale fit on a table about the size of this and produce flow rates of fractions of a pound-per-hour perhaps. They tend to be modules that are some small subset of the process, rather than the entire process concept.

Process development units are typically units that would fit in a room about the size of this hearing room and are typically a several million dollar investment to build. They are the workhorse of most industries when they are trying to do their first scaleup. Those tend to be fairly representative of systems for their scale.

Most good environmental work is done in PDU scale units. The next step up is pilot scale work, and I trust you are very familiar with many appropriations on pilot plants over the past 5 or 6 years. Those are very large units involving typically tens of millions of dollars of operating costs.

I do not believe personally that you need a proliferation of pilot plants in order to do good environmental work, but at the same time I do not believe you are going to do good environmental work at the bench.

The ambiguity in the administration's position is whether they really mean to settle in at PDU scale research or slide in down to bench. The budgets say benchscale work. You really do not have a proper budget to do the PDU.

Mr. **Fuqua**. Mr. Anthony?

Mr. **Anthony**. Mr. Chairman, I disagree a little bit with that statement. Those of us who are in the business of selling commercial equipment think in terms of demonstrating at about the 40, 50, 60 megawatt level. The reason for demonstrating at that level is that you have to develop design variables so that you can scale up to 300 or 600 megawatt sizes.

I will give you an example of that. We recently had difficulty with a new concept in DOE defined as the proof-of-concept stage—and my company was supplying the high-temperature, high-pressure precipitator for a PFBC project. The DOE felt satisfied that if that high-temperature, high-pressure precipitator worked once, then that was the proof-of-concept. Our argument was that you had to have it run for some length of time before you could be assured of the design variables. We came to some sort of an agreement on that.

That is the kind of thing that we look for.

Your committee, sir, has been very helpful in the past in that regard. I would like to think that the administration would follow your guidelines.

Dr. **Massey**. If I may, I would like to amplify.

Mr. **Fuqua**. Yes.

Dr. **Massey**. I have made a very clear distinction between high-risk R&D, which I believe proof of concept occurs where I have suggested, certainly environmentally, and the gap which may exist between that proof of concept and commercial demonstrations or commercial funds through SFC. I would not doubt for 1 minute that it is very ambiguous whether industry can afford to run demonstration facilities and properly then respond to SFC's requirements for commercial support. Whether that should be the mission of DOE or not, I guess that is really a policy decision of the Government. At the moment they have chosen——

Mr. **Fuqua**. It gets into a gray area.

Dr. **Massey**. I repeat, that is a gap in policy which increasingly exists as SFC stands where it stands and the evolution of fossil energy continues. What concerns me is not that gap. I do not think that is what you asked us to come to talk about. Rather, if you assume the charter of fossil energy, are we operating at a critical scale to make it an effective operation? I say no; I don't think so.

Mr. **Fuqua**. In order to better deal with acid rain, it is said that we should reduce our $SO_2$ emissions by about 90 percent. Yet, technologists and the economists say that somewhere between 50 and 60 percent is optional from technology and economical factors.

Is it worth the cost to go to a 90 percent reduction? Can we go that far technologically?

Dr. **Massey**. Mr. Chairman, I am an advocate of research. I do not just do research, but I believe in it. If the goal of research is to make that achievable at a greatly reduced cost compared with what our present capability is, I would thoroughly encourage——

Mr. **Fuqua**. Of course, once you get above 60 percent, the cost starts to escalate rather rapidly.

Dr. **Massey**. However, I think that is the pattern of technology. There is the learning curve, if you like, of technology in every instance.

If you were to ask me, is it a reasonable goal to work over the next 5 to 10 years to achieve at current costs substantially greater performance, I would say "Yes." In fact, I would go further and say I think we are headed into precisely the crisis that Toby described earlier on acid rain. I cannot personally imagine that we are not on our way to a policy on the management of acid rain. It may take us 5 years to fully establish what it should be, but it is going to immediately translate into a pressure on sulfur releases from coal combustion.

In the presumption that the policy of the Nation is to continue to use coal, there should be an answer to how we are going to get better performance on sulfur removal.

At the moment the administration's move in the R&D in that field, and I did look, by the way, at the direct coal-utilization part of the budget, is, as Toby says, a downward trend to ineffectiveness. I do not think you can count on that program if it is budgeted the way it is proposed now.

Mr. **Fuqua**. Thank you.

Mr. Bateman?

Mr. **Bateman**. Mr. Chairman, thank you.

I was intrigued with the indication that the Energy Research Advisory Board had come down on the negative side, or at least to some degree with reference to further Federal involvement with MHD. I would be very interested, Mr. Chairman, in obtaining the analysis or the assumptions that ERAB made in coming to that conclusion. If it is something they have stated as a conclusion without the benefit of analysis or the premises underlying the conclusion, I think we ought to try to probe into it further.

Is it possible that we will hear from ERAB with reference to this or might we request them to amplify their previous position?

Mr. **Fuqua**. We can attempt to do so. We have their report in which they just mention MHD, but they do not go into their reasons why they chose that. Now we did have some members of the ERAB panel before us. I will have to go back and look at the testimony. I do not remember if we talked to them specifically about MHD or not at that time.

Mr. **Bateman**. Perhaps staff might see if they have provided us with anything. If not, perhaps we might ask for some amplification on it.

Mr. **Fuqua**. Yes.

Mr. **Bateman**. Other than that, Mr. Chairman, I have no questions of the witnesses. I appreciated hearing from them and benefited from their testimony.

Mr. **Fuqua**. Thank you.

Mr. Boucher?

Mr. **Boucher**. Thank you, Mr. Chairman.

My first question is of Mr. Anthony.

You suggested during your testimony that, at a minimum, Congress should provide research and development funding which would encourage the conversion from oil to coal by industries and utilities. Could you comment on what particular technologies you think are most promising along that line and which, therefore, deserve our most concentration?

Mr. **Anthony**. Yes; in my testimony I indicated that our objective back in 1977 was to make coal substitutable for oil. I think that objective is still sound. Just from a strategic point of view, if you are going to try to eliminate the 4 or 5 million barrels a day of imported oil, that is the way you are going to do it.

The question then arrives at whether you utilize direct strategies or indirect strategies. For example, certainly if it were practical and economic for utilities in the Northeast to convert from oil to coal, that would be an indirect strategy that I think is still a valid one. It is a very complicated subject, though, as to whether the economics are there.

There are, I think, two or three instances where it has been done.

I would like to think there would be a Department of Energy game plan to make coal substitutable for oil, and then all of us look at that game plan that was a workable game plan.

Then there are the direct methods. For combustion, the liquefaction programs certainly would be substitutable. They have to be economic, though, in order for people to use them.

This coal-water mixture is a fascinating area. We do not do anything with it. We are not engaged in it, but I have read reports on it. The prospect with minor modification of boilers and combusters to use coal-water is fascinating. I think it even has more potential than coal-oil.

I would think in time—as a matter of fact, atmospheric fluidized bed combusters are now being sold at a pretty good rate. The boiler manufacturers are selling them. Every time you sell an atmospheric fluidized bed combuster, you are not going to sell an oil or gas combuster.

The indirect ways would be to encourage more use of coal in combustion and conversion to feedstock. The direct ways would be to fund liquefaction. I do not know anything about MHD or some of the other technologies.

Mr. **Boucher**. All right. Thank you, sir.

During the course of your statement you indicated that, with the proper effort, we could have a coal-based economy within 5 to 10 years. I would like, if you could, for you to give us some estimation of what you think the appropriate level of Government research and development funding just for coal technologies, not necessarily for other fossil fuel-type research, would help us achieve that reality within the timeframe you suggest.

Mr. **Anthony**. I think at minimum we should freeze it at 1982 levels. However, my guess is that if we had a technology guideline program, that you probably would end up doubling that amount of money in order to accomplish independence in 5 to 10 years.

I go through the DOE budget. I am sure there is a game plan somewhere, but that is not what I call a game plan. It seems that there is money here and there for a lot of purposes. I am talking about a game plan.

We are going to have so much coal being used for feedstock or converted to feedstock, so much to replace oil, so much for direct combustion. That is the kind of plan I think all of us need, that kind of guidance.

I do not know if that is a direct answer to your question.

Mr. **Boucher**. You are suggesting freezing funding at 1982 levels, which of course is far in excess of what the administration has recommended for 1984?

Mr. **Anthony**. Yes!

Mr. **Boucher**. I gather that your answer, then, is that we should fund at 1982 levels for fiscal year 1984. What would you project as an appropriate funding level for 1985, 1986, 1987, extending toward the date at which you think we might have a coal-based economy?

Mr. **Anthony**. I have not looked at that. I don't know. What we do need is breathing time to make that determination, as a country and as a Federal Government. That determination has to be made. I do not think I could make that determination because I do not know enough about all of the technologies.

Mr. **Boucher**. Would some other member of the panel like to comment on that? Mr. Irving?

Mr. **Irving**. A large number of industries are converting to coal, as in the electric utility industry. In the Northeast primarily, and in other areas as well, there are many boilers designed to burn coal that are now burning oil, in some cases gas. To convert these units back to coal-firing is within the technology, with the possible exception of improving the environmental area to the degree that may be required down the road, as Dr. Massey was mentioning.

People are converting. Our firm is and some other architectural engineers are doing so.

The key impediment to this conversion, as I see it, is the incentive to the utilities. If, for example, the utilities must bear the cost of the conversion, which can be hundreds of millions of dollars, without being able to pass at least part of this on to the customers who will benefit from it because of reduced fuel charges, they are not in a financial position to do it.

Some States at this point are encouraging conversion to coal and, as a method of doing it, are allowing the utility to pass on a percentage of the capital cost to conversion to the customers. I think this is a key factor that is holding up on the conversion to a coal economy.

Mr. **Boucher**. Do you have any suggestion as to what appropriate funding levels for fossil fuel research and development should be beyond 1984? I am interested, as Mr. Anthony is, in trying to determine what sort of game plan we can establish and whether making some substantial commitment to research and development in the 1984 budget should lead to that continuing commitment in the years ahead.

Do you have any suggestion as to whether 1982 levels would be sufficient for 1984 and for succeeding years, or whether some increases should be plotted as we go forward?

Mr. **Irving**. I cannot give you a direct answer. I think part of the answer, is where are we going in the nuclear area. In my personal opinion, we should be going nuclear and fossil about 50–50 on new construction. The decision or the direction of this split is going to affect whatever answer is given to you on what fossil R&D is necessary.

Mr. **Boucher**. Dr. Massey, would you like to comment?

Dr. **Massey**. I was just looking over the 1982 numbers for direct utilization. I trust your question is first directed at direct utilization of coal as contrasted with making liquids, and so on. The numbers are rather interesting.

We will come back to the heart of this: What scale of research are we doing? There was $24 million in fiscal year 1982 distributed over coal preparation, which is really coal cleaning; flue gas desulfurization, effectively hot gas cleanup in fluidized cycles; and waste treatment, and so forth. That budget, if you follow the administration, in 1984 would drop to $17 million. As part of that, there would be no solid waste work at all. They would essentially have most of those budgets. The single exception is their increasing on flue gas desulfurization.

I suspect, in trying to answer your question about moving forward, that to the extent that we are talking—first of all, I cannot conceive of 5 years as having a major effect, as I look at how long it takes to get any new invention beyond proven capability to the point where multiple people are buying. Five years is tomorrow afternoon. I would think in the timeframe of 10 to 15 that, if you acted now and had a directed research program, as Toby has referred to it, that you would substantially increase these numbers.

Where I come to difficulty is quantifying it, because you need a plan. The plan may be a very good one and only need twice as much money. However, at these funding levels I think we can discount the Government's role in direct coal utilization. It is not going to be there.

Mr. BOUCHER. Let me restrict the question somewhat and ask you what you think an appropriate R&D funding level for fossil fuels would be solely for fiscal year 1984.

Dr. MASSEY. I overlooked the point of scale of experimentation in my answer, and I will come back to it and answer your question.

If you read the language of the proposed administration budget, they are pulling out of every major prototype test. I would think that I disagree with that decision. Therefore, at a minimum, if you go back to that scale of testing, you are talking about the numbers that existed in 1982, which I trust is where Toby is coming from in his position. A minimum number would be in the order of magnitude of what we did in 1982.

By the way, the budget in 1983 is $27 million. It is not until this year that things are really fundamentally going down. 1982 and 1983 are the same number.

Mr. BOUCHER. Thank you.

Mr. Irving, I have one question for you, sir.

You indicated that the Soviets are presently designing a 500-megawatt MHD plant. Can you give us some indication of what other foreign countries are doing in this field and how the efforts that your organization is projecting compare with those attempts?

Mr. IRVING. At this time the efforts by other countries are minimal compared to the Soviet Union's. Japan has work going on. It is more benchscale, modified benchscale work. Australia has some. West Germany has some. Britain has little going on at this point.

The key efforts in MHD are in the Soviet Union and in the United States.

Mr. BOUCHER. How do we stand vis-a-vis the countries other than the Soviet Union? You mentioned Germany, I believe and Great Britain and Japan. How do our efforts stand compared to theirs?

Mr. IRVING. We are well advanced over the efforts in those other countries.

Mr. BOUCHER. That is encouraging.

Mr. IRVING. I might add, comparing ourselves with the Soviets, they approach this kind of development a lot differently than we do. They will spend tremendous effort in large units to gain research experience and test data compared to ourselves, and we will get the same results and better from a much smaller expenditure. It is a different approach.

Mr. BOUCHER. You have suggested about a $40 million R&D expenditure for fiscal year 1984. If that level of spending is approved, will that enable us to remain ahead of those other foreign nations, excluding the Soviet Union?

Mr. IRVING. Yes.

Mr. BOUCHER. All right, sir. Thank you.

Dr. Massey, I have one question for you.

You noted that sustained research and development funding at the level proposed by the administration for 1984 would result in the elimination of a number of test projects. Can you give us some indication of which of the test projects you believe would suffer the most greatly and would be in the most jeopardy?

Dr. MASSEY. I am not sure I can quickly give you that sort of specific, but I can approach it.

In the past 2 years, during this downturn in budgets, the fossil energy has shut down nearly every major pilot plant facility that it had in operation. In 1984, the Bi-Gas facility in Homer City is zeroed. The coal liquefaction efforts on two-stage liquefaction are zeroed out for 1984. I could go down the list. I am trying to find a survivor to suggest that there is something in the prototype or pilot scale to look at and operate. There is one left that comes to mind, and that is the fixed-bed, gas fire at Morgantown.

Every indication is that they are moving toward using that as a continuing test facility, much in the models of past years we have talked about GE as a test site, and so forth.

If these budgets were sustained past 1984, I would suspect that Morgantown may have a difficult time continuing to allocate those moneys. Therefore, Morgantown would probably also go down.

Mr. BOUCHER. That concludes the questions that I have.

That will conclude our hearing this morning.

I would like to thank the members of the panel who have provided some very thoughtful testimony to us. I can assure you that your presentations here today are very helpful to the subcommittee.

Thank you.

Mr. IRVING. Mr. Chairman, may we offer to respond in writing to your question regarding the funding of fossil——

Mr. BOUCHER. You may, Mr. Irving, and I would appreciate that.

The committee will stand adjourned.

[Whereupon, at 12:15 p.m., the subcommittee adjourned, to reconvene at the call of the Chair.]

# 1984 DEPARTMENT OF ENERGY AUTHORIZATION

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THURSDAY, MARCH 17, 1983

House of Representatives,
Committee on Science and Technology,
Subcommittee on Energy Development and Applications,
*Washington, D.C.*

The subcommittee met, pursuant to recess, at 9:34 a.m., in room 2318, Rayburn House Office Building, Hon. Don Fuqua (chairman of the subcommittee) presiding.

Present: Representatives Fuqua, Durbin, Nelson, Bouquard, Sensenbrenner, Bateman, and Schneider.

Staff present: Dr. Martha Krebs, staff director; John Ratzel, minority counsel; Bill Harvey, minority energy coordinator.

Mr. **Durbin** [acting chairman]. The Subcommittee on Energy Development and Applications will be convened.

Good morning. I would like at this point to invite our witnesses to come up: Mr. Dwain Spencer, director of advanced power systems division of the Electric Power Research Institute—good morning and welcome to the committee—and Mr. Peter Schwolsky, Washington representative of the Electric Power Research Institute. Good morning.

Today's hearing is the last in a series on the fiscal year 1984 Department of Energy fossil R&D budget. We will receive further testimony this morning from nongovernment witnesses with interests in fossil energy research and development programs.

Yesterday, we received near-unanimous support from industry witnesses for a continuing significant Federal role in R&D. This Nation's abundant fossil fuel resources are a key both to our future energy needs and our economic recovery. Finding ways to economically and cleanly use these resources must be a top national priority.

Just a few miles from my home in Springfield, Ill., are two of the world's most productive coal mines. One is opening on partial manpower and the other has threatened to close from time to time. At this very same time, the consumers in my home town, in my district, are paying record prices for other forms of energy. With the proper effort, this irony can be eliminated.

Today we expect to hear testimony providing additional insight regarding the proper role for Government funding of fossil energy R&D.

Before calling on our first witness today, I would ask unanimous consent that an opening statement by the Hon. F. James Sensenbrenner be placed in the record at this point.

[The opening statement of Mr. Sensenbrenner follows:]

### OPENING STATEMENT OF HON. F. JAMES SENSENBRENNER, JR.

### EDA SUBCOMMITTEE HEARING ON DOE PROGRAMS IN FOSSIL ENERGY FOR FISCAL YEAR 1984

Thank you, Mr. Chairman, for this opportunity to extend a welcome to our distinguished panel members.

This is the last of the hearings by the Subcommittee on the Research programs conducted by the Department of Energy. Today, we will consider the Fossil Energy programs conducted by the Department. The Administration has proposed significant reductions in these programs for fiscal year 1984. Our current economic situation mandates that we examine our Federal Energy Department programs to ensure that we fund only those programs that promise the highest potential for success and those that the private sector is unable to fund. As representatives of corporations and research organizations that have been involved in the development of Fossil Energy, your thoughts concerning the proposed and the impact that prior programs have had upon our economy and our energy supply, will be appreciated and useful in determining the appropriate level of funding for these programs.

I look forward to your testimony on this issue.

Mr. DURBIN. Mr. Spencer, would you like to summarize your written remarks for us, please?

## STATEMENT OF DWAIN F. SPENCER, DIRECTOR, ADVANCED POWER SYSTEMS DIVISION, ELECTRIC POWER RESEARCH INSTITUTE

Mr. SPENCER. Congressman Durbin, it is again a pleasure to address the subcommittee on specific elements of the Department of Energy's fiscal year 1984 fossil energy budget and to provide you a summary of some key demonstration projects under way, and finally to define what I consider to be a major unfulfilled demonstration need.

I am Dwain Spencer, director of the advanced power systems division of EPRI. In addition, I serve as a member of the Department of Energy's Fossil Energy Advisory Committee.

Although these remarks represent my own views, they are the result of EPRI's close relationship with the Department of Energy's fossil energy program over the last 6 years.

Although EPRI and the Department of Energy have been, and are, carrying on a number of cofunded projects in the fossil energy research and development area, the major reductions in funding for fossil energy R&D has placed a major added burden on the private sector to carry out the very expensive demonstration or pioneer plant step in the development of new process technology.

This altered emphasis within the Department of Energy has particularly impacted the continued development of coal liquefaction technologies, even though we have demonstrated technical readiness of at least two major processes during the last year. I am very concerned that these technical successes, and the experience related to them, will be lost if the country does not make a concerted effort to directly support the scale-up of at least one or two processes to the 3,000 to 5,000 tons per day scale.

Mr. Chairman, I would now like to expand further on this specific point.

During fiscal year 1983, the technical readiness of both the H-coal and Exxon donor solvent coal liquefaction processes was demonstrated through successful operation of two 250-ton-per-day pilot plants, jointly funded by the Department of Energy, EPRI, and other sponsoring organizations.

The H-coal pilot plant was operated on Kentucky, Illinois—in fact, No. 6 coal from your district—and Wyoming coals. EDS was operated also on Illinois No. 6 coals, Wyoming coals, and Texas lignite. Both processes produced satisfactory yields of crude oil from coal and demonstrated onstream factors comparable to normal petroleum refinery practices.

The total national investment in the development of these two processes exceeds $600 million of which approximately $450 million are Federal funds.

EPRI and many other organizations have carried out economic evaluations of the production costs and necessary selling prices for products from coal liquefaction facilities utilizing both normal financing approaches and specific synthetic fuels tax incentives. Without discussing these studies in detail, our estimates indicate that the initial selling price for coal-derived distillate fuels would range from $55 to $70 per barrel of product in 1982 dollars for mature commercial plants—that would be the fourth or fifth plant that would come on line—and be 25 percent to 40 percent greater for full-scale pioneer or demonstration plants, using 100 percent equity financing and what we would consider to be a minimum required return on investment.

If expensing of construction costs and highly leveraged financing were used, these initial prices could be reduced by about 20 to 33 percent.

As you know, the world market price for crude oil has been greatly reduced within the last 2 years as a result of our free world conservation efforts and the world recession. With crude oil prices, however, at $28 to $30 per barrel, the comparable product prices for distillate products from crude are about $36 to $40 a barrel as compared to the estimates that we have given you previously here of $55 to $70 for mature plants and probably more like $100 a barrel for the first pioneer plant.

Should crude prices further decline, the gap between coal liquids and petroleum-derived products would even further increase.

As a result, the petroleum community does not perceive any immediate incentive to proceed with pioneer or demonstration plant facilities. However, as I stated above, the technology base is nearly in hand and I, personally, believe it is in the best interests of the country to support one or two full-scale demonstrations of coal liquefaction technology.

It is likely that such support would require both direct outlays by the Department of Energy and significant tax incentives to stimulate private sector investment and to attract those organizations with the technical and management know-how to participate in the program.

Although it may be impractical to include any major funding for such pioneer or demonstration projects within the fiscal year 1984 fossil energy budget, perhaps seed monies for design of $10 to $20 million could be considered.

Since the operations at both the H-coal and EDS pilot plants have been terminated, a decision has been made to dismantle the Exxon donor solvent pilot plant and clear the site. I must say, Mr. Chairman, that EPRI did not support this decision, even though we were a participant in that project and offered to share in mothballing the plant.

In parallel, the ownership of the H-coal pilot plant has been transferred to Ashland Oil. We strongly recommend that the Department of Energy be provided funding of approximately $2 to $3 million to mothball the H-coal 250-ton-per-day pilot plant so that it could be available for future improvements in liquefaction technology; for example, those that are becoming evident from our Wilsonville pilot plant.

To this point, I have focused the presentation on H-coal and Exxon donor solvent processes. However, there are other coal liquefaction processes which should also be candidate for a demonstration project or projects.

Specifically, during the last year, very significant product yield improvements have been made with the highly integrated two-stage liquefaction process at the EPRI and Department of Energy jointly funded Wilsonville pilot plant. In fact, as a result of the closure of the DOE–SRC–II pilot plant and both the H-coal and Exxon donor solvent pilot plants, the Wilsonville 6-ton-per-day pilot unit is the largest operating coal liquefaction plant in the United States.

As you know, the Wilsonville facility is managed by the Southern Company Services Co. and operated by Catalytic, Inc. In fiscal year 1983, the research and development emphasis at the pilot plant was shifted to production of a substantially distillate product slate.

Thus far, the program has produced extremely promising results. Liquid yields over 60 percent have been attained, while hydrogen consumption per barrel was reduced by more than one third compared to either the H-coal or EDS processes. Hydrogen consumption is directly related to process efficiency and significantly impacts product production costs. We are presently conducting process economic studies to assess the cost implication of these improvements.

Further, we anticipate that further improvements can be made. As a result, EPRI has budgeted $3.5 million for the Wilsonville pilot plant during this calendar year, with the anticipation that the Department of Energy would continue providing operating funds of approximately $8 million in fiscal year 1984.

Our understanding from the Department of Energy is that they strongly support continuing the program at Wilsonville; therefore, I respectfully request your consideration to provide the necessary DOE funding for this project.

EPRI and the Southern Company are also in the process of defining a 3-year extension of the Wilsonville pilot plant program for 1984 through 1986 which would require DOE support at the $8 to $10 million per year level.

We hope that the continued technical achievements at Wilsonville will warrant this continued DOE funding support and that these process improvements continue to bring coal liquefaction nearer to commercial reality.

Finally, relative to coal liquefaction, I would like to comment on one final portion of the DOE program. The Department of Energy, Air Products and Chemicals, Fluor, and EPRI have jointly sponsored a pilot plant project to assess the liquid phase methanol synthesis process. EPRI studies have shown that such a process, in conjunction with an integrated coal gasification combined cycle powerplant, could produce methanol at costs competitive with other high quality turbine fuels. However, process experience to date is limited to small bench-scale process units, and scale up to a small pilot plant scale is necessary to more fully evaluate the process potential.

The present jointly funded project includes both bench-scale research and construction and operation of a 5-ton-per-day small pilot unit at La Porte, Tex. Construction of the pilot plant is scheduled to be complete by the end of September 1983, with operation scheduled by the end of this calendar year.

The fiscal year 1984 DOE budget does not contain funds to continue this project, and the private sector sponsors are not in a position to assume this obligation. The necessary DOE funds for this project are estimated to be $4.6 million in fiscal year 1984 in order to maintain project schedule and momentum. Again, we understand that DOE supports the technical program; however, funding limitations have precluded them from including this effort in their budget submission.

I would like to just submit for the record my comments relative to power generation. They emphasize, Congressman Durbin, the fact that we believe that there should be continued development of advanced coal utilization technologies that show promise of high efficiency, of production of electricity from coal, and those that emphasize the utilization of our very high sulfur bituminous coals such as those from the region which you represent.

I would like, then, to leave that section and summarize, finally, with the key demonstration project activities, which I think you may find of interest.

I would like to update you on two significant demonstration projects which are continuing within the United States. The first is the cool water 100-megawatt coal gasification project, and the second is the Heber 45-megawatt binary cycle geothermal demonstration project.

Both of these projects have greatly benefited from the actions of this committee, the former through recent actions of the Synthetic Fuels Corporation and the latter through the direct support of this committee, which certainly we greatly appreciate.

Detailed engineering of the 100-megawatt cool water full-scale module plant is over 90-percent complete, and plant construction is now over 30-percent complete. The estimated project cost of $294 million has remained firm for over 4 years, with over $20 million of contingency funds remaining.

As you know, this project is EPRI's largest financial obligation, with EPRI funding of over $100 million. During this last year, the project applied for a price guarantee from the Synthetic Fuels Corporation to cover unexpected reductions in revenues. These reduced revenues were a direct result of oil price reductions and would have resulted in an unacceptably high financial risk during the plant operating period.

I am very pleased to state that the Synthetic Fuels Corporation has indicated that they plan to issue a letter of intent for a price guarantee to the project in the near future. This action by the Synthetic Fuels Corporation assures us the ability to operate the facility for at least 5 years, with the intent the plant will have a total operating lifetime of 20 years.

With this assistance from the Synthetic Fuels Corporation, the cool water partnership now plans to add a second spare gasifier with a quench system. Site and plant engineering have always anticipated this possibility, so only moderate changes to the plant will be required. In fact, we expect that this spare gasifier will be in place at the initiation of operations in June of next year. However, it may be a few months before it is fully commissioned.

Even with the reductions in oil and gas prices, EPRI's economic studies indicate that integrated coal gasification combined cycle powerplants can be competitive with other base-load electricity options and meet stringent environmental emission requirements.

Clearly, the successful operation of the cool water plant would provide a firm basis for commercial commitments from the power industry.

One last key point is that during the last year, we carefully evaluated the cost of such plants as a function of scale and are now convinced that unit additions of 250 megawatts or greater can be economically competitive, thus providing the electric utility industry with another degree of freedom in addition of plant capacity.

I simply will add here, Mr. Chairman, that we have now recently established a new organization called the Utility Coal Gasification Associates of which Dr. Robert Uhrig of Florida Power and Light is the initial chairman. This group has the responsibility now of working with EPRI in order to enhance the communication of our demonstration results to the utility industry on a timely basis, so that they will be in a position, we hope within a few years, to make commercial commitments to the first coal gasification combined cycle plants for the power production in this country.

We think that is a very important step beyond the cool water plant in enhancing the understanding and developing the confidence in the utility industry that these technologies can play a significant commercial role.

This flexibility in plant capacity additions is also germane to my last subject, the Heber binary cycle demonstration plant. As you know, this project is also jointly funded among DOE, San Diego Gas and Electric, EPRI, and other utilities and State agencies. Detailed engineering is now over 50-percent complete; all major hardware procurements are in place or have firm bids; and a heat sales agreement for the geothermal brine has been negotiated.

We were all extremely pleased when the chairman of this committee, Mr. Fuqua, participated in the formal groundbreaking ceremonies for the plant, and we trust that he perceived a strong commitment that all parties have to this project.

EPRI has recently projected the cost of power from 50-megawatt commercial units which would be based on the Heber project results and find that the power costs from such plants would be directly competitive with coal plants in California and less expensive than either gas- or oil-fired capacity.

Further, since geothermal plants can be added in increments of 50 megawatts, the utility can even more closely match its projected loads with capacity additions.

In summary, I believe that developing the capability for the electric utility industry to add modular power systems in the 1990's is an important research, development, and demonstration objective for EPRI and the Nation. Further, the Nation should posture itself to be in a position to contend with the downside contingency; that is, high oil prices or curtailed supply.

Conversion of coal to liquid and gaseous fuels, coupled to high efficiency power cycles, appears to be one major approach for addressing this contingency.

I believe a modest increase in the Department of Energy's fossil energy fiscal year 1984 budget of approximately $80 million to $100 million in the specific technical areas which I have identified could make a significant difference in our preparedness.

Finally, I believe that the Federal Government must reconsider the importance of its financial role in the support of demonstration-scale plants for technologies which are technically sound but whose projected economic value is uncertain. This risk-sharing concept may require many forms; however, I do not believe that present approaches are sufficient to establish the broad-based synthetic fuels technical and industrial base that the Nation should have.

Thank you very much for this opportunity to present my views to this committee.

[The prepared statement of Mr. Spencer follows:]

# REMARKS ON THE DEPARTMENT OF ENERGY FY'84 BUDGET AND ON A SUMMARY OF KEY DEMONSTRATION PROJECTS AND NEEDS

Dwain F. Spencer
Director
Advanced Power Systems Division

Hearing on FY1984 Authorization for the
Department of Energy Fossil Energy Research
and Development Program

Presented to:

Subcommittee on Energy Development and
Application

Committee on Science and Technology
United States House of Representatives
Washington, D.C.

17 March 1983

## I) Introduction

Mr. Chairman, it is again a pleasure to address the subcommittee on specific elements of the Department of Energy's FY84 Fossil Energy budget, to provide you a summary of some key demonstration projects underway and finally to define a major unfilled demonstration need. I am Dwain Spencer, Director of the Advanced Power Systems Division of the Electric Power Research Institute (EPRI). In addition, I serve as a member of the Department of Energy's Fossil Energy Advisory Committee. Although these remarks represent my own views, they are the result of EPRI's close relationship with the DOE fossil energy program for over six years.

Although EPRI and the Department of Energy have, and are, carrying on a number of cofunded projects in the fossil energy research and development area, the major reductions in funding for fossil energy R&D/D has placed a major added burden on the private sector to carry out the very expensive demonstration or Pioneer plant step in the development of new process technology. This altered emphasis within the Department of Energy has particularly impacted the continued development of coal liquefaction technologies even though we have demonstrated technical readiness of at least two major processes during the last year. I am very concerned that these technical successes, and the experience related to them, will be lost if the country does not make a concerted effort to directly support the scale-up of at least one or two processes to the 3000-5000 ton/day scale. Mr. Chairman, I would now like to expand further on this point.

## II) Specific Remarks on the DOE FY'84 Fossil Energy Program and It's Relationship to Previously Funded Projects

### A. Coal Liquefaction

During FY'83, the technical readiness of both the H-Coal and Exxon Donor Solvent coal liquefaction processes was demonstrated through successful operation of two 250 ton/day pilot plants, jointly funded by the Department of Energy, EPRI, and other sponsoring organizations. The H-Coal pilot plant was operated on Kentucky, Illinois, and Wyoming coals and the Exxon Donor Solvent Process on Illinois and Wyoming coals and Texas lignite. Both processes produced satisfactory yields of crude oil from coal and demonstrated on stream factors comparable to normal petroleum refinery practices. The total national investment in the development of these two processes exceeds $600 million of which approximately $450 million are federal funds.

Although large scale tests have not yet been conducted to convert the vacuum tower bottoms to synthesis gas for either hydrogen production or fuel gas, partial oxidation tests at TVA and RuhrChemie are planned for FY'84, and we believe such tests will be successful. Therefore, by the end of FY'84, a firm design basis for designing commercial scale coal liquefaction modules on at least two processes should be available.

EPRI and many other organizations have carried out economic evaluations of the production costs and necessary selling prices, for products from coal liquefaction facilities utilizing both normal financing approaches and specific synthetic fuels tax incentives. Without discussing these studies in detail, our estimates indicate that the initial selling price for coal derived distillate fuels would range from $55 to $70 per barrel of product in 1982 dollars for mature commercial plants and be 25 percent to 40 percent greater for full scale Pioneer or demonstration plants, using 100 percent equity financing and minimum required return on investment. Expensing construction costs and highly leveraged financing could reduce these prices by 20 to 33 percent.

As you know, Mr. Chairman, the world market price for crude oil has been greatly reduced within the last two years, as a result of free world conservation efforts and the world recession. With crude oil prices at $28-30 per barrel the comparable product prices are $36 to $40 per barrel. Should crude prices further decline, the gap between coal liquids prices and petroleum derived products would even further increase.

As a result, the petroleum community does not perceive any immediate incentive to proceed with Pioneer or demonstration plant facilities. However, as I stated above, the technology base is nearly in hand and I, personally, believe it is in the best interests of the country to support one or two full scale demonstrations of coal liquefaction technology. It is likely that such support would require both direct outlays by the Department of Energy and significant tax incentives to stimulate private sector investment and to attract those organizations with the technical/management know-how to participate in the program. Although it may be impractical to include any major funding for such Pioneer or demonstration projects within the FY'84 Fossil Energy budget, perhaps seed monies for design of $10 to $20 million could be considered.

Since the operations at both the H-Coal and EDS pilot plants have been terminated, a decision has been made to dismantle the EDS pilot plant and clear the site. I must state that EPRI did not support this decision and offered to share in mothballing costs. In parallel, the ownership of the H-Coal pilot plant has been transferred to Ashland Oil, Inc. We strongly recommend that DOE be provided funding of approximately \$2-3 million to mothball the H-Coal pilot plant, so that it could be available for future improvements in liquefaction technology, for example those that are becoming evident from the Wilsonville pilot plant.

To this point, I have focused this presentation on the H-Coal and Exxon Donor Solvent processes; however there are other coal liquefaction processes which should also be candidates for such a demonstration project(s). Specifically, during this last year, very significant product yield improvements have been made with a highly integrated two stage liquefaction process at the EPRI-DOE jointly funded Wilsonville pilot plant. In fact, Mr. Chairman, as a result of the closure of the DOE-SRC-II pilot plant and both the H-Coal and EDS pilot plants, the Wilsonville six ton/day small pilot plant is the largest operating coal liquefaction plant in the U.S.

As you know, the Wilsonville facility is managed by the Southern Company Services Company and operated by Catalytic, Inc. In FY1983, the research and development emphasis, at the pilot plant, was shifted to production of a substantially distillate product slate. Thus far, the program has produced extremely promising results. Liquid yields over 60 percent have been attained, while hydrogen consumption per barrel was reduced by more than one third compared to either the H-Coal or EDS processes. Hydrogen consumption is directly related to process efficiency and significantly impacts product production cost. We are presently conducting process economic studies to assess the cost implications of these improvements.

Further, we anticipate that further improvements can be made. As a result, EPRI has budgeted $3.5 million for the Wilsonville pilot plant in CY1983, with the anticipation that the Department of Energy would continue providing operating funds of approximately $8.0 million in FY'84. Our understanding from the DOE is that they strongly support continuing the program at Wilsonville; therefore I respectfully request your consideration to provide the necessary DOE funding for this project.

EPRI and the Southern Company are in the process of defining a three year extension to the Wilsonville pilot plant (1984-86) which would require DOE support at the $8-$10 million per year level. We hope that the continued technical achievements at Wilsonville will warrant this continued DOE funding support and that these process improvements continue to bring coal liquefaction nearer to commercial reality.

Finally, Mr. Chairman, I would like to comment on one final portion of the DOE coal liquefaction program. DOE, Air Products and Chemicals, Inc., Fluor and EPRI have jointly sponsored a pilot plant project to assess the Liquid Phase Methanol synthesis process. EPRI studies have shown that such a process, in conjunction with an integrated coal gasification combined cycle power plant, could produce methanol at costs competitive with other high quality turbine fuels. However, process experience, to date, is limited to small bench scale process units, and scale-up to a small pilot plant scale is necessary to more fully evaluate the process potential.

The present jointly funded project includes both bench scale research and construction and operation of a five ton per day small pilot plant at La Porte, Texas. Construction of the pilot plant is scheduled to be complete by the end of September, 1983, with operation scheduled by the end of the calendar year.

The FY'84 DOE budget does not contain funds to continue this project, and the private sector sponsors are not in a position to assume this obligation. The necessary DOE funds for this project are estimated to be $4.6 million in FY'84 in order to maintain project schedule and momentum. Again, we understand that DOE supports the technical program; however, funding limitations have precluded them from including this effort in their budget submission.

### B. Power Generation

My final comments on the DOE-FE program pertain to advanced power generation technologies, namely the High Temperature Turbine Technology, Open Cycle Magnetohydrodynamic (OCMHD) and Fuel Cell programs. As I stated in my testimony to you last year, I believe these programs should be continued because they show promise of more efficiently utilizing coal for electric power production. Nearly all energy forecasts now project that coal is likely to play an even more important role in power generation throughout this century and well into the next. As one assesses the costs of electricity from such plants, over 30 years, the price of the feed coal is the dominant economic factor. Therefore, there is already an economic incentive to develop advanced coal technologies which show promise of achieving coal to busbar efficiencies of 40 to 50 percent.

Specifically, I recommend that DOE-FE continue to support an advanced gas turbine program, utilizing either water cooling or advanced air cooling techniques, at an annual level of approximately $20 million per year. This is the minimum level necessary to design, fabricate and test these concepts even in the Laboratory. EPRI has focused its funding in this area on improving the reliability/availability of the next improved commercial air cooled turbines; therefore is not in a position to sponsor these more advanced options.

Again, consistent with my recommendations of a year ago, DOE should continue supporting the OCMHD activities at the Component Development and Integration Facility (CDIF) in Montana, the University of Tennessee Space Institute, and the necessary subsystem development programs to provide hardware for testing at these facilities. During the last year, the EPRI inverter facility at the CDIF has been commissioned and the initial performance tests on the inverter have been very successful. These tests were performed with ash laden oil firing; however, if DOE support continues, a coal fired combustor will be installed at the CDIF this year. I continue to believe that a federal investment of $35-$40 million annually in this technology is warranted and recommend such funding.

Although I also support continued research and development on advanced fuel cells, I will not comment specifically on that area, as I am not as technically familiar with that technology.

C. Key Demonstration Projects

Mr. Chairman, I would now like to update you on two significant demonstration projects which are continuing within the U.S. The first is the Cool Water 100 MWe coal gasification project and the second, the Heber 45 MWe binary cycle geothermal demonstration project. Both of these projects have greatly benefitted through the actions of this committee, the former through recent actions of the Synthetic Fuels Corporation and the latter through the direct support of this committee.

Detailed engineering of the 100 MWe Cool Water full scale module plant is over 90 percent complete and plant construction is now over 30 percent complete. The estimated project cost of $294 million has remained firm with over $20 million of contingency funds remaining. As you know, this Project is EPRI's largest financial obligation, with EPRI funding of over $100 million. During this last year, the project applied for a price guarantee from the Synthetic Fuels Corporation, to cover unexpected reductions in revenues. These reduced revenues were a direct result of oil price reductions and would have resulted in an unacceptably high financial risk during the plant operating period.

I am extremely pleased, Mr. Chairman, to state that the Synthetic Fuels Corporation has indicated that they plan to issue a Letter of Intent for a price guarantee to the project in the near future. This action by the SFC assures us the ability to operate the facility for at least five years, with the intent that the plant will have a total operating lifetime of 20 years.

With this assistance from the SFC, the Cool Water Partnership now plans to add a second spare gasifier with a quench system. Site and plant engineering have always anticipated this possibility, so only moderate changes to the plant will be required. In fact, we expect that the spare gasifier will be in place at the initiation of operations in June 1984; however, it may be a few months before it is fully commissioned.

Even with the reductions in oil and gas prices, EPRI's economic studies indicate that integrated coal gasification combined cycle power plants can be competitive with other base-load electricity options and meet stringent environmental emission requirements. Clearly the successful operation of the Cool Water plant would provide a firm basis for commercial commitments from the power industry. One last key point, is that during the last year, we carefully evaluated the cost of such plants as a function of scale, and are now convinced that unit additions of 250 MWe or greater can be economically competitive; thus providing the electric utility industry with another degree of freedom in addition of plant capacity.

This flexibility in plant capacity additions is also germane to my last subject, namely the Heber binary cycle demonstration project. As you know, this project is also jointly funded among DOE, San Diego Gas and Electric, EPRI, and other utilities and state agencies. Detailed engineering is now over 50 percent complete, all major hardware procurements are in place or have firm bids, and a heat sales agreement for the geothermal brine has been negotiated. We were all extremely pleased, Mr. Chairman, with your participation in the formal ground breaking ceremonies for the plant and trust that you perceived the strong commitment that all parties have to this project.

EPRI has recently projected the cost of power from 50 MWe commercial units which would be based on the Heber project results, and find that the power costs from such plants would be directly competitive with coal plants in California and less expensive than either gas or oil fired capacity. Further, since geothermal plants can be added in increments of 50 MWe, the utility can even more closely match its projected loads with capacity additions.

## D. Summary

In summary, I believe that developing the capability for utilities to add "Modular Power Systems in the 1990s" is an important R&D/D objective for EPRI and the nation. Further, the nation should posture itself to be in a position to contend with the downside contingency, i.e., high oil prices or curtailed supply. Conversion of coal to liquid and gaseous fuels, coupled to high efficiency power cycles, appears to be one major approach for addressing this contingency.

I believe a modest increase in the DOE-FE FY'84 budget of approximately $80 to $100 million in the specific technical areas which I have identified, could make a significant difference in our preparedness. Finally, I believe that the federal government must reconsider the importance of its financial role in the support of demonstration scale plants for technologies which are technically sound, but whose projected economic value is uncertain. This risk sharing concept may require many forms; however I do not believe that present approaches are sufficient to establish the broad based synthetic fuels technical/industrial base that the nation should have.

Thank you very much, Mr. Chairman, for the opportunity to address your subcommittee.

ELECTRIC POWER RESEARCH INSTITUTE

EPRI

## BIOGRAPHY

Dwain F. Spencer

Division Director
Advanced Power Systems

Mr. Spencer is the Director of the Advanced Power Systems Division at the Electric Power Research Institute. He joined EPRI in April 1974 as Program Manager of Solar and Geothermal Energy and in 1975 became Technical Manager of Planning and Analysis for the Division. In April 1976, he was promoted to Director, Advanced Fossil Power Systems Department. He was previously on loan from Caltech to the National Science Foundation during the period 1972-74 where he served as Program Manager, Solar Thermal Conversion.

Mr. Spencer had been employed by the Caltech Jet Propulsion Laboratory since 1958. His most recent position was Project Manager, Space Technology Applications which included management of projects in the application of science and technology to problems in urban engineering, medical engineering, law enforcement assistance and environmental management. Immediately prior to that assignment, Mr. Spencer was responsible for the technical development of a broad range of hardware systems for the Viking Mars Entry and Landing Mission.

Mr. Spencer holds a B.S. in Chemical Engineering from Notre Dame, Masters Degree in Engineering from Purdue, and has completed extensive additional course work in Nuclear Engineering at U.C.L.A. He is a member of Sigma Xi, Tau Beta Pi, and the American Institute of Chemical Engineers. He also serves as a member of the Department of Energy's Fossil Energy Advisory Committee.

Mr. Spencer is the author of over 40 technical publications including numerous papers on advanced nuclear propulsion systems, space systems research and development and development of advanced solar, geothermal, and coal based power systems.

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Mr. DURBIN. Thank you, Mr. Spencer.

I would like now to ask Mr. Schwolsky to make a few remarks related to his testimony.

I would like to caution you in advance, Mr. Schwolsky, that both you and I are in very tenuous positions this morning: we may be the only people in the room not wearing green, and that could be widely misunderstood. But Mr. Spencer, with his background at Notre Dame, will cover for us. [Laughter.]

I would appreciate it now if you would summarize your remarks.

## STATEMENT OF PETER M. SCHWOLSKY, WASHINGTON COUNSEL, ELECTRIC POWER RESEARCH INSTITUTE

Mr. SCHWOLSKY. Indeed, Mr. Chairman, he is big enough for both of us.

I thank you for this opportunity to speak before the subcommittee.

I am here today as Washington counsel for EPRI, speaking in behalf of Mr. Kurt Yeager, who is director of the coal combustion systems division at EPRI. Unfortunately, as your staff is aware, Mr. Yeager is unable to be here today because of a preexisting commitment.

With your permission, I would like to summarize the major points of his testimony and submit his testimony in its entirety for the record.

Mr. DURBIN. That will be fine.

Mr. SCHWOLSKY. Thank you.

National energy research and development should be based on the premise that the Nation's economy will continue to grow. Our R&D efforts must therefore provide the tools to assure that the economy is not constrained by an inadequate energy supply.

Recent events have not diminished EPRI's concern about the future adequacy of electricity supply. We must, however, keep clearly in mind our goal of long-term economic growth and the fundamental importance of energy to that goal. Doing so provides a clear path for our planning. Let us examine a few of the guideposts on this path:

One, the link between electricity demand and GNP remains constant.

Two, reduced dependence on oil will remain an important national objective.

Three, coal powerplants will generate the largest share of electricity required over the next three decades.

Four, increased productivity will be essential for economic growth.

Five, the electric utility industry's financial condition will be a major factor in determining future electricity supply and reliability.

Six, advanced technologies will account nationally for only a small share of electricity generation this century. Utilities will have to expand the usefulness of current equipment and facilities and depend on improved conventional coal-combustion technologies for the bulk of their new installations.

Seven, the domestic manufacturing and supply capability supporting energy utilization is threatened with rapid erosion.

Eight, and the final point, our society will continue to have a strong environmental ethic. Sound scientific data derived from independent, peer-reviewed research must be developed in a timely manner on topics of environmental concern. Current environmental control capabilities must be improved by reducing capital and operating costs and preserving or improving overall plant availability.

The challenge resulting from these issues is to develop equipment and techniques which help the Nation weather its current economic difficulties without sacrificing opportunities which will be essential in the 1990's and beyond. A strategy of balance, prudence, and flexibility will best serve the Nation in an era where the uncertainties may continue to be great.

Some of the key objectives with respect to coal-combustion-related technology should be the following: Provide improved characterization of our domestic coal resources; develop improved coal quality control and assessment technology; improve the performance and longevity of conventional coal-fired installations; improve the cost and reliability of environmental control technology; develop clean, modular, quickly sited new coal-fired technology which will reduce the cost of energy utilization to both consumer and stockholder; and, last, provide economic and regulatory incentives to insure the rapid commercial utilization of technology improvements.

As to specific program impacts that the fiscal year 1984 DOE budget will have, my comments will focus on three major program categories reflecting Federal efforts in coal combustion related R&D.

These are: (1) control technology and coal preparation; (2) advanced research and technology development; and, (3) combustion systems.

The fiscal year 1984 DOE request for these three categories totals approximately $55,990,000. This is a 37 percent reduction from the 1983 budget and a 54 percent reduction from 1982.

With regard to combustion systems and, more specifically, atmospheric fluidized bed combustion, the DOE program has been instrumental in AFB achieving the capability to reliably produce steam from coal for small industrial and space-heating applications. The program has included installation of several industrial demonstrations and is an example of the success that can be achieved through a well planned and consistent Government research, development, and demonstration program conducted in a manner complementing private-sector capabilities and objectives.

Based on the success of this technology development, EPRI and the utility industry are moving aggressively to implement commercial scale—that is, 100 to 200 megawatt—demonstrations of AFB. EPRI is presently evaluating proposals from five separate utilities, representing a wide range of company sizes, industry sectors, and geographical regions.

In each case, the host utility is proposing to fund the largest portion of the cost of the demonstration, with EPRI and the boiler manufacturers sharing the first-of-a-kind risk cost with the utility.

The continuity and success of this combined Federal/private development effort may make this pioneer demonstration step for AFB commercially viable for the private sector. This is, however, likely to be the exception rather than the rule, unless Federal support for high risk coal combustion R&D, including critical pilot plants, is maintained at a substantially greater level than requested in fiscal year 1984.

Turning to pressurized fluidized bed combustion, the impact of the proposed DOE funding reduction is nowhere more immediately evident than in the PFB development program. Major progress has been made over the past several years in converting this technology from an engineering curiosity to an important electric-power-producing technology having the lowest bus bar energy potential cost of any near-term coal utilization option. The effective cancellation of DOE support on the brink of successful proof of commercial viability is indeed seizing defeat from the jaws of victory.

The uncertain and constrained utility market also influences pressurized fluidized bed combustion development goals. The influence arises from the trend toward smaller unit size and growing utility interest in uprating the capacity of existing units to bring capacity on line at the lowest investment cost. As a result, development emphasis is being placed on the turbocharged PFB boilers which can provide shop-fabricated, barge-transportable steam generation modules. These can be rapidly field erected to provide the desired uprating in unit sizes of 150 to 250 megawatts. This approach will also use coal to replace and increase the capacity of existing oil- or gas-fired plants while minimizing space and environmental control requirements. In addition, the turbocharged boiler uses a technically simpler gas turbine configuration which only drives the boiler compressor and is not used to generate electricity.

Efforts are now being initiated by EPRI to develop engineering designs for the PFB turbocharged boiler based on results from operational pilot plants. This approach should permit commercial prototypes to be operational this decade, thus yielding important financial advantages to utilities during the current period of low-load growth and high construction costs. The combination of shorter leadtime and small unit cost provided by the turbocharged boiler may substantially reduce capital costs for generations.

The major limiting factor restricting commercial realization of this capability is confidence in the design base for the PFB boiler. Although many of the technical issues affecting the gas turbine and balance of plant have been reduced or eliminated by the turbocharged boiler approach, the boiler remains the critical pacing development issue. Under these conditions, I am very concerned by the proposal to terminate Federal support for the International Energy Agency Grimethorpe pilot PFB facility. I believe no single issue is more critical to the commercial success of PFB than continued operation of Grimethorpe. Grimethorpe is the only operational PFB facility in the world which has the technical capability and size to adequately address the critical pacing development issue involving boiler confidence. It has been the cornerstone for development of PFB. In addition, it is supported by the largest concentration of PFB experience in the world. Its results are nonproprietary and are available to participants in each supporting nation. In short, its capabilities are irreplaceable and represent one of the most valuable applications of Federal energy R&D funding.

I therefore strongly recommend that the United States support a streamlined Grimethorpe project to be continued for at least the next 2 years. Failure to do so will cut us off from our most valuable source of development capability and stall the private sector's efforts to achieve the technical and commercial advantages of PFB.

Based on the success of Grimethorpe and other PFB development programs, EPRI plans to implement a commercial turbocharged PFB prototype with the utility industry this decade. I also emphasize that such a commercial demonstration will probably only be implemented if the Grimethorpe results indicate, to users' satisfaction, confidence in the reliability and operability of PFB powerplants.

In addition to the budget impacts on AFBC and PFBC, there are some other programmatic areas of concern. First, greater Federal emphasis should be placed on resolving the technical issues limiting the effectiveness of mandated environmental control technology. The limitations, combined with uncertainty over the future course of regulatory requirements, pose one of the greatest impediments to increased coal use.

Second, the alternative fuel utilization program sponsored by DOE is essential, particularly in the area of coal-water mixtures. This work, together with the efforts sponsored by EPRI and others in the private sector, will provide the necessary technical confidence for commercialization of this important oil replacement option. Development of techniques to produce stable, pumpable mixtures of coal and water offer the possibility of a substantial fuel cost saving that could justify conversion of oil-designed boilers to coal.

And, third, longer range, higher risk coal programs which should also receive higher priority for continued Federal R&D support are materials research, direct coal utilization, and coal preparation and analysis. Of particular importance in the latter category is higher confidence in our coal reserve quality and quantity estimates and improved quality control techniques for our lower ranked coals.

In conclusion, I would like to say that there is no shortage of opportunities to improve the efficiency, reliability, and environmental performance of coal combustion. Successful achievement of the objectives laid out in this testimony requires, however, a more constructive and coordinated partnership between government and the private sector. This encompasses the planning and implementation of R&D as well as the commercial transfer of results. Furthermore, the rate of commercialization and use of improved technology also depends on the development of regulatory and economic incentives which encourage introduction of innovative technology. Too often, our dependence on the adversarial approach to resolution makes the conflicts restricting coal use more severe and disruptive than necessary and restricts the introduction of technological improvement.

I want to thank you for this opportunity to testify in behalf of Mr. Yeager.

[The prepared statement of Mr. Yeager, given by Mr. Schwolsky, follows:]

STATEMENT BY

KURT E. YEAGER
DIRECTOR, COAL COMBUSTION SYSTEMS DIVISION
ELECTRIC POWER RESEARCH INSTITUTE
PALO ALTO, CA

BEFORE THE
SUBCOMMITTEE ON ENERGY DEVELOPMENT AND APPLICATIONS
COMMITTEE ON SCIENCE AND TECHNOLOGY
U. S. HOUSE OF REPRESENTATIVES

MARCH 17, 1983

## INTRODUCTORY STATEMENT

MR. CHAIRMAN, MEMBERS OF THE SUBCOMMITTEE:

I AM KURT E. YEAGER, DIRECTOR OF THE COAL COMBUSTION SYSTEMS DIVISION OF THE ELECTRIC POWER RESEARCH INSTITUTE (EPRI). I APPRECIATE THIS INVITATION TO MEET WITH YOU TO DISCUSS THE EFFECT OF PROPOSED CHANGES IN THE FEDERAL PROGRAM OF COAL COMBUSTION RELATED RESEARCH AND DEVELOPMENT. THESE CHANGES HAVE SIGNIFICANT IMPLICATIONS FOR THE ELECTRIC UTILITY INDUSTRY AND THEREFORE ARE OF GREAT IMPORTANCE TO EPRI.

THE ELECTRIC POWER RESEARCH INSTITUTE WAS ESTABLISHED BY THE ELECTRIC UTILITY INDUSTRY IN 1972 TO CONDUCT A BROAD RESEARCH AND DEVELOPMENT PROGRAM FOR THE ENTIRE ELECTRIC UTILITY INDUSTRY AND ITS RATE PAYERS. EPRI'S R&D MISSION IS TO DEVELOP NEW AND IMPROVED TECHNOLOGIES FOR ELECTRIC POWER PRODUCTION, DELIVERY AND USE, AND TO HELP ACCELERATE THEIR AVAILABILITY TO INSURE AN ADEQUATE SUPPLY OF ECONOMIC, RELIABLE AND ENVIRONMENTALLY ACCEPTABLE ELECTRICITY.

EPRI IS SUPPORTED BY VOLUNTARY CONTRIBUTIONS FROM INVESTOR-OWNED, GOVERNMENT-OWNED AND COOPERATIVELY-OWNED ELECTRIC UTILITIES ACROSS THE NATION. OVER THE NEXT FIVE YEARS, EPRI WILL DIRECT ABOUT $360 MILLION TO IMPROVING DIRECT COAL COMBUSTION SYSTEMS. THESE EFFORTS WILL SPAN COAL QUALITY CONTROL, POWER PLANT PERFORMANCE AND RELIABILITY, FLUIDIZED BED COMBUSTION AND ENVIRONMENTAL CONTROL.

IN TERMS OF ALLOCATION OF THE EPRI COAL COMBUSTION SYSTEMS DIVISION'S EFFORT, THE LARGEST PART OF OUR WORK (70-80%) IS FOCUSED ON THE NEXT TEN YEARS, PROVIDING IMPROVEMENTS IN THE PERFORMANCE AND RELIABILITY OF EXISTING POWER PLANTS AND ENVIRONMENTAL CONTROL. THE REMAINDER OF THE DIVISION'S EFFORT IS DEVOTED TO NEXT GENERATION COAL COMBUSTION TECHNOLOGY, ESPECIALLY FLUIDIZED BED COMBUSTION AND COAL SLURRY FUELS.

SINCE ITS INCEPTION, EPRI HAS WORKED WITH THE DEPARTMENT OF ENERGY (AND ITS PREDECESSOR AGENCIES), THE ENVIRONMENTAL PROTECTION AGENCY, AND OTHER FEDERAL AGENCIES WITH ENERGY RESEARCH AND DEVELOPMENT RESPONSIBILITIES TO ACHIEVE MUTUAL TECHNICAL OBJECTIVES.

MY TESTIMONY TODAY WILL FOCUS ON JOINT AND COORDINATED EPRI/DOE ACTIVITIES, AND THE EFFECTS OF FEDERAL PROGRAM CHANGES ON THE DEVELOPMENT OF DIRECT COAL COMBUSTION TECHNOLOGY. THESE COMMENTS WILL AUGMENT AND UPDATE MR. YEAGER'S EARLIER TESTIMONY TO THE SUBCOMMITTEE ON MARCH 23, 1982.

## STRATEGIC CONSIDERATIONS

NATIONAL ENERGY RESEARCH AND DEVELOPMENT (R&D) SHOULD BE BASED ON THE PREMISE THAT THE NATION'S ECONOMY WILL CONTINUE TO GROW. WITHIN THE NEXT TWO DECADES WE EXPECT A BETTER TRAINED, BETTER EDUCATED, AND MORE EXPERIENCED WORK FORCE, WHICH WILL BE AT LEAST ONE-THIRD LARGER THAN TODAY. TO ACHIEVE THE EXPECTATIONS OF THESE WORKERS, REAL NATIONAL INCOME WILL HAVE TO INCREASE ACCORDINGLY. OUR R&D EFFORTS MUST THEREFORE PROVIDE THE TOOLS TO ASSURE THAT THE ECONOMY IS NOT CONSTRAINED BY AN INADEQUATE ENERGY SUPPLY.

RECENT EVENTS HAVE NOT DIMINISHED EPRI'S CONCERN ABOUT THE FUTURE ADEQUACY OF ELECTRICITY SUPPLY. A SLUGGISH ECONOMY, HIGH INTEREST RATES, AND THE WEAKENED FINANCIAL HEALTH OF MANY OF OUR INDUSTRIES MAKE PLANNING AN APPROPRIATE RESEARCH AND DEVELOPMENT PROGRAM DIFFICULT IN THE SHORT TERM. WE MUST, HOWEVEV, KEEP CLEARLY IN MIND OUR GOAL OF LONG TERM ECONOMIC GROWTH AND THE FUNDAMENTAL IMPORTANCE OF ENERGY TO THAT GOAL. DOING SO PROVIDES A CLEAR PATH FOR OUR PLANNING. LET US EXAMINE A FEW OF THE GUIDEPOSTS ON THIS PATH.

1. THE LINK BETWEEN ELECTRICITY DEMAND AND GNP REMAINS CONSTANT. IN THE POST-OIL EMBARGO PERIOD THERE HAS BEEN A GENER UNCOUPLING OF THE STRONG HISTORICAL LINK BETWEEN ECONOMIC GROWTH AND

TOTAL ENERGY USE. SIGNIFICANTLY, HOWEVER, THERE HAS BEEN A REMARKABLE CONTRAST AMONG TRENDS IN TOTAL ENERGY, ELECTRICITY AND THE ECONOMY. IN 1980, FOR EXAMPLE, THERE WAS A SHARP DROP IN TOTAL ENERGY USE REFLECTING THE 1979 OIL PRICE INCREASES. ELECTRICITY USE, HOWEVER, CLIMBED BECAUSE INDUSTRY INCREASINGLY SUBSTITUTED ELECTRICITY FOR OTHER ENERGY FORMS WHILE RESIDENTIAL AND COMMERCIAL CUSTOMERS MADE GREATER DEMANDS. AS A RESULT, ELECTRICITY IS ASSUMING A GROWING SHARE OF THE ENERGY PICTURE, HAVING INCREASED FROM 15% OF TOTAL U.S. ENERGY IN 1950 TO 33% TODAY AND 45% EXPECTED BY THE END OF THE CENTURY.

2. REDUCED DEPENDENCE ON OIL WILL REMAIN AN IMPORTANT NATIONAL OBJECTIVE. THE U.S. STILL DEPENDS ON SIGNIFICANT IMPORTS OF OIL. THIS HAS BOTH ECONOMIC AND SECURITY IMPLICATIONS. FOR EXAMPLE, RISKS OF SUPPLY INTERRUPTIONS TO EUROPE AND JAPAN REQUIRE CAREFUL CONSIDERATION OF U.S. DEMANDS ON AVAILABLE PETROLEUM SUPPLIES. UNFORTUNATELY, SHORT-TERM SUPPLY AND DEMAND RELATIONSHIPS ARE AGAIN CREATING THE MYTH OF THE LONG-TERM OIL GLUT. CONSUMERS' HEARTS LEAP AS THEY SEE THE PRICE OF A GALLON OF GAS DIP BELOW A DOLLOR. HAPPY DAYS ARE HERE AGAIN AND WE CAN RELAX OUR NATIONAL ENERGY POLICY AND PRIORITIES ACCORDINGLY. THE REALITY, HOWEVER, IS FAR MORE COMPLEX. THE LONGER TERM REALITIES OF SUPPLY AND DEMAND IN MY OPINION WILL NOT SUPPORT A DRASTIC PRICE BREAK. OIL PRICES CAN BE EXPECTED TO REMAIN SOFT IN THE EARLY 1980'S, THEN RISE AS THE WORLD ECONOMY PICKS UP AND SUPPLIES ARE DEPLETED. THIS RISE WILL PROBABLY CONTINUE TO BE ERRATIC BASED ON RECURRING CRISES IN INTERNATIONAL POLITICS. IN THIS UNCERTAIN WORLD WE SHOULD NOT BE LULLED BACK INTO ACCEPTING OUR DANGEROUS DEPENDENCE ON UNDEPENDABLE ENERGY SOURCES. OUR NATIONAL ENERGY POLICY MUST PROVIDE CONSISTENT, LONG-TERM INSULATION FROM THE RECURRING HISTORY OF IMPORTED OIL DISLOCATIONS. STABLE ENERGY COSTS, AS WE LEARNED THE HARD WAY, ARE ESSENTIAL TO ECONOMIC GROWTH. IN ORDER TO ASSURE STABILITY WE MUST BASE OUR ENERGY ECONOMY

ON OUR ABUNDANT DOMESTIC ENERGY SOURCES - COAL AND NUCLEAR.

3. COAL POWER PLANTS WILL GENERATE THE LARGEST SHARE OF ELECTRICITY REQUIRED OVER THE NEXT THREE DECADES. COAL TODAY PROVIDES OVER 50% OF THE ELECTRICITY GENERATED IN THE U.S. ALMOST ALL NEW CAPACITY BUILT OVER THE REMAINDER OF THIS CENTURY WILL USE COAL. OIL AND GAS-FIRED CAPACITY WILL DECREASE ACCORDINGLY. PROVIDING THE CAPACITY NECESSARY TO PRODUCE THE ELECTRICITY REQUIRED TO SUPPORT REASONABLE ECONOMIC EXPECTATIONS AND MAKE BEST USE OF OUR LARGE COAL AND URANIUM RESERVES WILL BE A MAJOR NATIONAL CHALLENGE. ELECTRICITY IS THE BEST WAY WE HAVE TO MAKE THESE ABUNDANT DOMESTIC RESOURCES AVAILABLE TO OUR ECONOMY. EMPHASIS NEEDS TO BE PLACED ON DEVELOPING ELECTRIC POWER AS A SUBSTITUTE FOR OIL, NOT ONLY IN STATIONARY USES BUT ULTIMATELY FOR OUR TRANSPORTATION REQUIREMENTS AS WELL.

4. INCREASED PRODUCTIVITY WILL BE ESSENTIAL FOR ECONOMIC GROWTH. IMPROVEMENTS IN THE PRODUCTIVE USE OF ENERGY WILL BE REQUIRED THROUGH THE END OF THE CENTURY AND BEYOND. ELECTRICITY WILL BE INCREASINGLY DEPENDED ON TO ACHIEVE INCREASED NATIONAL PRODUCTIVITY. AS A RESULT, NEW, MORE PRODUCTIVE END-USE TECHNOLOGIES WILL FURTHER INCREASE THE DEMAND FOR ELECTRICITY AS THEY DISPLACE OTHER, MORE COSTLY TECHNIQUES.

5. THE ELECTRIC UTILITY INDUSTRY'S FINANCIAL CONDITION WILL BE A MAJOR FACTOR IN DETERMINING FUTURE ELECTRICITY SUPPLY AND RELIABILITY. ECONOMIC INSTABILITY IS A FORMIDABLE RESTRAINT TO BALANCED NATIONAL ENERGY PLANNING. THE WEAK FINANCIAL STATE OF THE ELECTRIC UTILITY INDUSTRY IS NOT SOME TEMPORARY ABERRATION OF THE MARKET; INSTEAD, IT HAS BEEN YEARS IN THE MAKING, BROUGHT ABOUT BY FUNDAMENTAL CHANGES IN THE TOTAL COST OF PRODUCING ELECTRICITY COUPLED WITH THE PERSISTENT FAILURE OF THE REGULATORY SYSTEM TO RESPOND EFFECTIVELY TO THESE CHANGES. THE RESULT IS AN UNHAPPY SITUATION, WITH RATEPAYERS ANGERED AND CONFUSED BY EVER-RISING ELECTRICITY

BILLS AND SHAREHOLDERS FRUSTRATED BY INADEQUATE RETURNS. MORE AND MORE, UTILITIES MUST FOCUS FIRST ON CONTROLLING PRESENT-DAY CAPITAL EXPENDITURES AND SECOND ON PROVIDING THE PUBLIC WITH ADEQUATE, RELIABLE, LEAST-COST SERVICE OVER THE LONG TERM. THIS AVOIDANCE OF CAPACITY EXPANSION AT THE EXPENSE OF ADEQUATE FUTURE POWER SUPPLY RUNS COUNTER TO THE LONG TERM INTEREST OF CONSUMERS AND UTILITIES. MORE FUNDAMENTALLY IT WILL SEVERELY CONSTRAIN OUR ECONOMY. WE MUST SEEK TO BUILD A STRONGER FOUNDATION OF PUBLIC UNDERSTANDING ON WHY AN ADEQUATE SUPPLY OF ELECTRICITY IS SO NECESSARY TO ECONOMIC GROWTH, JOB CREATION, IMPROVED PRODUCTIVITY AND NATIONAL SECURITY. IT SHOULD NOT TAKE CHRONIC POWER SHORTAGES OR THE BANKRUPTCY OF AN INDUSTRY TO GIVE THIS ISSUE THE ATTENTION IT DESERVES.

6. ADVANCED TECHNOLOGIES WILL ACCOUNT NATIONALLY FOR ONLY A SMALL SHARE OF ELECTRICITY GENERATION THIS CENTURY. UTILITIES WILL HAVE TO EXPAND THE USEFULNESS OF CURRENT EQUIPMENT AND FACILITIES AND DEPEND ON IMPROVED CONVENTIONAL COAL COMBUSTION TECHNOLOGIES FOR THE BULK OF THEIR NEW INSTALLATIONS. MORE ADVANCED TECHNOLOGIES WILL ONLY BEGIN TO PENETRATE THE UTILITY GENERATION MIX BY 1990. BY THE YEAR 2000 WE WOULD EXPECT ABOUT 10% OF ELECTRICITY GENERATION WILL COME FROM THESE NEW SOURCES.

7. THE DOMESTIC MANUFACTURING AND SUPPLY CAPABILITY SUPPORTING ENERGY UTILIZATION IS THREATENED WITH RAPID EROSION. 1982 ENDED WITH THE LOWEST AMOUNT OF NEW GENERATING CAPACITY ORDERED IN OVER 40 YEARS. THIS IS IN SHARP CONTRAST TO THE OVER 50,000 $MW_E$ OF CAPACITY WHICH WERE ORDERED JUST A DECADE AGO AND THE OVER 30,000 $MW_E$ OF ANNUAL DOMESTIC MANUFACTURING CAPACITY WHICH EXISTS TODAY. UNFORTUNATELY, THERE IS LITTLE HOPE THAT AN IMMEDIATE RECOVERY WILL OCCUR IN THE SLUGGISH ECONOMIC CLIMATE FACING THE UTILITY INDUSTRY. UNDER THESE MARKET CIRCUMSTANCES, DEPRESSED DOMESTIC EQUIPMENT SUPPLIERS FIND IT INCREASINGLY DIFFICULT TO JUSTIFY MAINTAINING THEIR UTILITY AND RELATED ENERGY

CAPABILITY, LET ALONE INVEST IN NEW TECHNOLOGY DEVELOPMENT. AS A RESULT, WE RUN CONSIDERABLE RISK, IN MY JUDGEMENT, OF SURRENDERING OUR HISTORIC LEADERSHIP IN ENERGY TECHNOLOGY DEVELOPMENT AND MARKETS TO OTHER NATIONS WHO ENCOURAGE A MORE CONSTRUCTIVE AND CONSISTENT RELATIONSHIP BETWEEN GOVERNMENT AND INDUSTRY.

IT IS ESSENTIAL THAT THE U.S. MORE AGGRESSIVELY ENCOURAGE THE COMPETITIVENESS OF OUR DOMESTIC SUPPLIERS IN THE INTERNATIONAL MARKET. FAILURE TO DO SO WILL NOT ONLY CONCEDE OUR NATIONAL TECHNICAL LEADERSHIP BUT MORE IMPORTANTLY, LEAVE US DEPENDENT ON FOREIGN SOURCES OF SUPPLY FOR OUR ENERGY INDUSTRIES, ESPECIALLY THE ELECTRIC UTILITY INDUSTRY. THE STRATEGIC AND ECONOMIC IMPLICATIONS OF THIS CONTINUING EROSION OF OUR INTERNATIONAL COMPETITIVENESS ARE OBVIOUSLY ENORMOUS. FOR EXAMPLE, BEGINNING IN 1976 WE HAVE SEEN SEVEN CONSECUTIVE MERCHANDISE TRADE DEFICITS, GROWING TO AN EXPECTED $75 BILLION THIS YEAR. UNLESS WE LEARN AS A NATION TO INCREASE THE RATE OF NET INVESTMENT IN PLANT AND EQUIPMENT, THE PROCESS OF INCREASING CAPITAL INTENSITY WHICH HAS MARKED OUR HISTORIC ECONOMIC GROWTH, WILL BE STALLED. NOT ONLY WILL OUR COMPETITIVENESS CONTINUE TO ERODE BUT ANY INCREASE IN PRODUCTIVITY AND REAL INCOME WILL BE IMPOSSIBLE.

8. OUR SOCIETY WILL CONTINUE TO HAVE A STRONG ENVIRONMENTAL ETHIC. SOUND SCIENTIFIC DATA DERIVED FROM INDEPENDENT, PEER-REVIEWED RESEARCH MUST BE DEVELOPED IN A TIMELY MANNER ON TOPICS OF ENVIRONMENTAL CONCERN. CURRENT ENVIRONMENTAL CONTROL CAPABILITIES MUST BE IMPROVED BY REDUCING CAPITAL AND OPERATING COSTS AND PRESERVING OR IMPROVING OVERALL PLANT AVAILABILITY. APPROXIMATELY 40% OF THE CAPITAL INVESTMENT AND 30% OF THE TOTAL COST OF POWER FOR NEW, COAL-FIRED POWER PLANTS ARE RELATED TO ENVIRONMENTAL CONTROL. THIS INCLUDES SULFUR DIOXIDE ($SO_2$) SCRUBBING, PARTICULATE CONTROL, SOLID WASTE DISPOSAL, WATER TREATMENT AND PLANT COOLING. THESE CONTROLS IN THEIR CURRENT FORM ARE ESTIMATED TO REDUCE NET PLANT EFFICIENCY BY 3 TO 5% AND HAVE BEEN ONE OF THE GROWING CONTRIBUTORS TO THE LOSS OF AVAILABILITY OF EXISTING

COAL-FIRED PLANTS. CLOSELY ASSOCIATED WITH THIS ISSUE IS THE INCREASING DIFFICULTY IN SITING NEW UTILITY AND OTHER ENERGY FACILITIES. TECHNOLOGIES WHICH COMBINE REDUCED ENVIRONMENTAL IMPACT AND WATER UTILIZATION WITH THE ABILITY TO SITE NEAR DEMAND CENTERS SHOULD BE GIVEN INCREASED PRIORITY.

## RESEARCH OBJECTIVES AND ISSUES

THE CHALLENGE RESULTING FROM THESE ISSUES IS TO DEVELOP THE EQUIPMENT AND TECHNIQUES WHICH HELP THE NATION WEATHER ITS CURRENT ECONOMIC DIFFICULTIES WITHOUT SACRIFICING OPPORTUNITIES WHICH WILL BE ESSENTIAL IN THE 1990'S AND BEYOND. A STRATEGY OF BALANCE, PRUDENCE AND FLEXIBILITY WILL BEST SERVE THE NATION IN AN ERA WHERE THE UNCERTAINTIES MAY CONTINUE TO BE GREAT.

SOME OF THE KEY OBJECTIVES WITH RESPECT TO COAL COMBUSTION RELATED TECHNOLOGY SHOULD BE THE FOLLOWING:

o PROVIDE IMPROVED CHARACTERIZATION OF OUR DOMESTIC COAL RESOURCES.

o DEVELOP IMPROVED COAL QUALITY CONTROL AND ASSESSMENT TECHNOLOGY.

o IMPROVE THE PERFORMANCE AND LONGEVITY OF CONVENTIONAL COAL-FIRED INSTALLATIONS.

o IMPROVE THE COST AND RELIABILITY OF ENVIRONMENTAL CONTROL TECHNOLOGY.

o DEVELOP CLEAN, MODULAR, QUICKLY SITED NEW COAL-FIRED TECHNOLOGY WHICH WILL REDUCE THE COST OF ENERGY UTILIZATION TO BOTH CONSUMER AND STOCKHOLDER.

o PROVIDE ECONOMIC AND REGULATORY INCENTIVES TO INSURE THE RAPID COMMERCIAL UTILIZATION OF TECHNOLOGY IMPROVEMENTS.

AGAIN THIS YEAR THE PROPOSED DOE BUDGET PLACES PRIMARY RESPONSIBILITY FOR TECHNOLOGY INNOVATION AND DEVELOPMENT ON THE PRIVATE SECTOR. THE SUCCESS OF THIS POLICY DEPENDS ON UNDEFINED ECONOMIC AND REGULATORY INCENTIVES TO MAKE TECHNOLOGY DEVELOPMENT

ATTRACTIVE TO THE PRIVATE SECTOR. UNTIL THESE INCENTIVES ARE BETTER DEFINED FOR THE ECONOMICALLY FRAGILE AREA OF COAL COMBUSTION R&D, OPPORTUNITIES IN COAL QUALITY IMPROVEMENT, FLUIDIZED BED AND OTHER COAL COMBUSTION ADVANCES AS WELL AS ENVIRONMENTAL CONTROL TECHNOLOGY WILL CONTINUE TO BE CONSTRAINED FROM TIMELY DEVELOPMENT AND APPLICATION. ALL OF THESE TECHNOLOGIES CAN HAVE MAJOR IMPACTS ON THE QUANTITY AND QUALITY OF COAL UTILIZATION OVER THE NEXT 20 TO 30 YEARS.

THE ISSUE BECOMES CRITICAL WHEN IT IS NECESSARY TO BUILD LARGE-SCALE DEMONSTRATIONS. THESE ARE ESSENTIAL ELEMENTS IN PROVING OUT THE ECONOMICS AND TECHNICAL ASPECTS OF NEW TECHNOLOGIES. IT IS OFTEN NECESSARY TO GAIN EXPERIENCE WITH SEVERAL SUCH LARGE-SCALE PROJECTS BEFORE THE RISKS ARE REDUCED TO THE LEVEL OF BROAD COMMERCIAL ACCEPTABILITY. EPRI AND OTHER PRIVATE SECTOR ORGANIZATIONS ARE CAPABLE OF PARTICIPATING IN THESE EFFORTS BUT NOT ASSUMING SOLE RESPONSIBILITY FOR THEIR SPONSORSHIP. THIS IS FURTHER EXACERBATED BY THE SLUGGISH ECONOMIC POSTURE THROUGHOUT THE PRIVATE SECTOR.

THUS, THERE ARE A NUMBER OF PROGRAMS IN COAL COMBUSTION RELATED TECHNOLOGY FOR WHICH THE FUNDING MECHANISM TO ACHIEVE COMMERCIALIZATION IS NOT EVIDENT. FURTHERMORE, DIFFICULTIES IN MAINTAINING ADEQUATE EMPHASIS ON SUPPORTING RESEARCH, FOR WHICH GOVERNMENT FUNDING IS ALSO BEING REDUCED, POSES A LONGER TERM THREAT TO THE TECHNOLOGICAL BASE NECESSARY FOR TECHNICAL AND COMMERCIAL ENERGY LEADERSHIP.

## SPECIFIC PROGRAM IMPACTS

MY COMMENTS WILL FOCUS ON THREE MAJOR PROGRAM CATEGORIES REFLECTING FEDERAL EFFORTS IN COAL COMBUSTION RELATED R&D. THESE ARE (1) CONTROL TECHNOLOGY AND COAL PREPARATION, (2) ADVANCED RESEARCH AND TECHNOLOGY DEVELOPMENT AND (3) COMBUSTION SYSTEMS. THE FY 1984 DOE REQUEST FOR THESE THREE CATEGORIES TOTALS $55,990,000, A 37% REDUCTION FROM THE 1983 BUDGET AND A 54% REDUCTION FROM 1982.

## A. CONTROL TECHNOLOGY AND COAL PREPARATION

### 1. COAL PREPARATION AND ANALYSIS

THE DOE PROGRAM APPEARS AIMED AT PRODUCING ULTRA-CLEAN COAL AS A FUEL TO COMPETE DIRECTLY WITH RESIDUAL FUEL OIL AND HIGHER QUALITY OIL FUELS, SUCH AS NO. 2 FUEL OIL AND DIESEL FUEL. DOE IS PROPOSING TO REDUCE THE FY 1984 COAL PREPARATION PROGRAM FROM $8 TO $5 MILLION. THIS IS INCONSISTENT WITH THE STATED INTENT OF THEIR PROGRAM; DEVELOPMENT OF PROCESSES (REALLY CHEMICAL COAL CLEANING PROCESSES) FOR PRODUCTION OF ULTRA-CLEAN COAL. A VERY SIGNIFICANT R&D EFFORT WOULD BE REQUIRED TO ECONOMICALLY PRODUCE THIS ULTRA-CLEAN COAL. SPECIFIC AREAS REQUIRING SIGNIFICANT RESEARCH ARE: COAL COMMINUTION (WAYS OF COMMINUTING COAL TO MICRON SIZES WITHOUT EXCESSIVE ENERGY CONSUMPTION ARE A MAJOR REQUIREMENT FOR ULTRA-CLEAN COAL PRODUCTION); COAL MINERAL MATTER REACTION CHEMISTRY (CURRENTLY WE HAVE A VERY POOR UNDERSTANDING OF THE ACTUAL FORMS IN WHICH MINERAL MATTER OCCURS IN COAL LET ALONE HOW IT REACTS IN CHEMICAL PROCESSES); COAL ORGANIC SULFUR CHEMISTRY; COAL SURFACE PROPERTIES; GENERAL COAL CHEMISTRY; AND CONSTRUCTION MATERIALS DEVELOPMENT.

THE DOE PROGRAM WITH REGARD TO COAL CLEANING IS COMPLEMENTARY WITH EPRI GOALS. DOE IS PLANNING TO DIRECT ITS COAL CLEANING PROGRAM TOWARD LONG TERM DEVELOPMENT OF FUELS FOR THE INDUSTRIAL AND POSSIBLY TRANSPORT SECTORS. THE EPRI COAL QUALITY PROGRAM IS CONCERNED WITH THE UTILITY SECTOR, EMPHASIZING DEVELOPMENT THAT WILL RESULT IN NEAR-TERM COST SAVINGS TO THE UTILITY INDUSTRY.

PRIORITY OBJECTIVES FOR JOINT RESEARCH AND DEVELOPMENT TO IMPROVE COAL CLEANABILITY FORECASTS AND TECHNOLOGY INCLUDE:

- IMPROVED SAMPLING AND ANALYTICAL PROCEDURES TO INSURE CONFIDENCE IN COAL RESERVE QUANTITY AND QUALITY ESTIMATES.
- TECHNIQUES FOR CLEANING AND CONTROLLING THE QUALITY OF LOWER RANK WESTERN COALS.
- MORE EFFICIENT AND ECONOMICAL FINE COAL CLEANING AND RECOVERY TECHNIQUES.
- MORE ECONOMICAL CHEMICAL CLEANING TECHNIQUES PROVIDING THOROUGH SULFUR AND ASH REMOVAL.
- AUTOMATED CLEANING PROCESS CONTROL AND ON-LINE CONTINUOUS MEASUREMENT TECHNIQUES.

2. FLUE GAS CLEANUP

SEVERAL YEARS AGO, FEDERAL LEADERSHIP FOR THE DEVELOPMENT AND EVALUATION OF FLUE GAS CLEANUP SYSTEMS WAS TRANSFERRED FROM EPA TO DOE. THIS WAS APPLAUDED AS A PROGRESSIVE STEP PROMOTING RESOLUTION OF THE RELIABILITY, PERFORMANCE AND BY-PRODUCT ISSUES LIMITING THE EFFECTIVENESS OF MANDATED CONTROL TECHNOLOGY. UNFORTUNATELY, TO DATE VERY LITTLE SUBSTANTIVE PROGRESS HAS BEEN ACHIEVED, ALTHOUGH MANY OPPORTUNITIES FOR IMPROVEMENT EXIST AND COSPONSORED PROJECTS HAVE BEEN DEVELOPED PENDING DOE PARTICIPATION.

THE OPERATIONAL LIMITATIONS ASSOCIATED WITH THE REQUIRED FLUE GAS CLEANUP TECHNOLOGY, TOGETHER WITH UNCERTAINTY OVER THE FUTURE COURSE OF REGULATORY REQUIREMENTS ARE MAJOR IMPEDIMENTS TO INCREASED COAL USE. IT IS, THEREFORE, DISTURBING THAT THE GOVERNMENT MAINTAINS A POSITION WHICH EFFECTIVELY RESTRICTS ITS COOPERATION IN SOLVING TECHNICAL LIMITATIONS IN THE TECHNOLOGY IT HAS MANDATED. BY CHANGING THE DOE PROGRAM CONTENT TO LONG RANGE TECHNOLOGY IT CAN NO LONGER CONTRIBUTE TO SOLVING THE ENVIRONMENTAL CONSTRAINTS LIMITING COAL UTILIZATION IN THIS DECADE OR PROBABLY THE NEXT.

RECOMMENDED AREAS WHERE CONCENTRATED R&D EFFORT IS REQUIRED INCLUDE:

- IMPROVING THE RELIABILITY OF EXISTING SLURRY (LIME/ LIMESTONE) SCRUBBING-SYSTEMS FOR SULFUR DIOXIDE REMOVAL.
- REDUCING THE QUANTITY AND IMPROVING THE QUALITY OF DISPOSABLE BY-PRODUCTS ASSOCIATED WITH SLURRY SCRUBBING SYSTEMS.
- DEMONSTRATING REGENERABLE FLUE GAS DESULFURIZATION SYSTEMS WHICH MINIMIZE THE USE OF EXPENSIVE REAGENTS, PRODUCE MARKETABLE BY-PRODUCTS AND RELY ENTIRELY ON COAL.
- DEVELOPING AND DEMONSTRATING COMPACT FLUE GAS CLEAN-UP SYSTEMS SUPPORTING THE CONVERSION OF EXISTING OIL-FIRED POWER PLANTS TO COAL.
- DEVELOPING SIMPLIFIED FLUE GAS CLEANUP SYSTEMS CAPABLE OF SIMULTANEOUS REMOVAL OF SULFUR AND NITROGEN OXIDES.

THE EFFECTIVENESS OF GOVERNMENT FUNDING IN THIS CATEGORY WILL DEPEND ON A MUCH MORE AGGRESSIVE R&D POLICY WHICH FOCUSES ON THE KEY PROBLEM AREAS INDICATED AND COMMITS JOINTLY WITH THE PRIVATE SECTOR TO THE MAJOR TECHNOLOGY PROJECTS REQUIRED FOR THEIR RESOLUTION. BY COMPARISON, EPRI ALONE IS INVESTING ANNUALLY IN THIS AREA MORE THAN TWICE THE FY 1984 DOE REQUEST.

### 3. GAS STREAM CLEANUP

THE MOST CRITICAL APPLICATION OF GAS STREAM CLEANUP TO THE DIRECT COMBUSTION OF COAL IS IN PRESSURIZED FLUIDIZED BED COMBUSTION (PFB). THE TECHNICAL AND COMMERCIAL SUCCESS OF THIS TECHNOLOGY AS AN ELECTRIC POWER GENERATING SYSTEM DEPENDS ON THE ABILITY OF THE GAS TURBINE TO OPERATE RELIABLY ON THE DUST LADEN GAS STEAM PRODUCED BY THE PFB.

THE DOE PROGRAM PROVIDES MUCH OF THE R&D BASIS FOR THE NEW AND YET UNPROVEN CLEANUP TECHNOLOGY THAT WILL BE REQUIRED TO ACHIEVE A VIABLE PFB ELECTRIC POWER GENERATING SYSTEM. AS A RESULT, CONSIDERABLE PROGRESS HAS BEEN MADE DURING THE LAST YEAR IN THE TECHNOLOGY OF GAS FILTRATION AT ELEVATED TEMPERATURES AND PRESSURES. VALUABLE SCIENTIFIC AND ENGINEERING INFORMATION HAS BEEN OBTAINED ON SEVERAL TYPES OF FILTERS OVER A RANGE OF PFB COMBUSTION GAS CONDITIONS.

PFB IS ONE CLEAR EXAMPLE WHERE DOE EFFORTS HAVE BEEN INSTRUMENTAL IN DEVELOPING AN IMPORTANT, BUT HIGH RISK, NEW COAL UTILIZATION TECHNOLOGY NEARLY TO THE POINT WHERE TRANSFER FOR PRIVATE SECTOR COMMERCIALIZATION IS FEASIBLE. TO SUBSTANTIALLY REDUCE SUPPORT NOW ON THE MAJOR REMAINING TECHNICAL PROBLEMS IS TO RISK NEGATING THE PFB INVESTMENT TO DATE.

### 4. CLEANUP BASE TECHNOLOGY

UNFORTUNATELY, DOE IS AGAIN PROPOSING TO TERMINATE THEIR WORK IN CLEAN-UP BASE TECHNOLOGY. THEIR WORK IN THE DISPOSAL AREA (CHARACTERIZATION OF COAL POWER PLANT WASTE AND ECONOMIC ASSESSMENT OF THE EFFECTS OF RCRA) HAS BEEN EFFECTIVE. AN EXTENSIVE DATA BASE ON WASTE CHARACTERISTICS AND CURRENT PRACTICE HAS BEEN PRODUCED.

I RECOMMEND THAT DOE REMAIN IN THE UTILIZATION AREA. PRIVATE SECTOR EFFORTS ARE MOVING TOWARD ESTABLISHING A SOUND BASIS FOR CHOOSING ASH AS AN ALTERNATIVE RAW MATERIAL FOR ROAD BASES, LAND FILL, STRUCTURAL AGGREGATE, AND CEMENT SUBSTITUTES. THIS MAY REQUIRE SEVERAL LARGE DEMONSTRATIONS TO DEVELOP GENERIC (RATHER THAN SITE SPECIFIC) INFORMATION. THE USING INDUSTRIES (HIGHWAY/BUILDING CONSTRUCTORS, ETC.) ARE FRAGMENTED. NO EASILY IDENTIFIED COMMERCIAL ENTITY EXISTS TO FUND SUCH WORK BUT THE COST SAVINGS TO THE NATION COULD BE SUBSTANTIAL.

SIMILARLY, RESOURCE FROM ASH (PARTICULARLY ALUMINUM, IRON, VANADIUM) HAS THE POTENTIAL FOR MEETING A LARGE PORTION OF OUR NEEDS WHICH ARE CURRENTLY MET BY IMPORTS. MANY OF THE BENEFITS (BALANCE OF PAYMENTS, FREEDOM FROM FOREIGN SOURCES, NATIONAL SECURITY, PRICE STABILITY) ARE DIFFICULT FOR PRIVATE COMMERCIAL FUNDING SOURCES TO QUANTIFY AND SHOW AN ECONOMIC RETURN. SUPPORT OF SUCH OBJECTIVES IS APPROPRIATE AND PERHAPS ESSENTIAL.

## B. ADVANCED RESEARCH AND TECHNOLOGY

THE PROGRAMS OF GREATEST CONCERN ARE FIRST, MATERIALS. GOVERNMENT SUPPORT AND FACILITIES FOR THE RESEARCH, DEVELOPMENT AND VALIDATION OF NEW AND IMPROVED MATERIALS HAS BEEN FUNDAMENTAL TO THE TECHNOLOGICAL LEADERSHIP THE NATION HAS ENJOYED. THE LONG-TERM, NON-PROPRIETARY BENEFITS TO THE NATION AND THE SCOPE OF REQUIRED EFFORT, HOWEVER, EXCEED THE INVESTMENT RISK WHICH THE PRIVATE SECTOR ALONE CAN JUSTIFY. IN MY JUDGEMENT, THERE IS NO MORE COST-EFFECTIVE APPLICATION OF FEDERAL R&D SUPPORT WHICH IS ALSO ENTIRELY CONSISTENT WITH CURRENT FEDERAL R&D POLICY. THE RIGOROUS ENVIRONMENTS IN WHICH BOTH NEW COAL COMBUSTION AND CONVERSION TECHNOLOGIES MUST OPERATE, AS WELL AS THE NEED FOR IMPROVED PERFORMANCE AND RELIABILITY FROM EXISTING POWER PLANTS, IS PLACING DRASTIC NEW DEMANDS ON OUR MATERIALS SCIENCE AND ENGINEERING BASE. FUNDING FOR THIS PROGRAM SHOULD BE ACCELERATED, NOT REDUCED.

THE SECOND AREA OF MAJOR CONCERN IS DIRECT UTILIZATION. THE NEARLY 60% PROPOSED REDUCTION IN DOE EFFORT INFERS THAT THE TECHNOLOGY BASE FOR INCREASED UTILIZATION OF COAL, ESPECIALLY LOW RANK COAL AND COAL-LIQUID MIXTURES, IS WELL ESTABLISHED. IN MY JUDGEMENT SUCH IS NOT THE CASE. AREAS PARTICULARLY MERITING CONTINUED EFFORT INCLUDE:

- CHARACTERIZATION OF COAL ASH CHEMISTRY AND ITS EFFECT ON COMBUSTION.
- STANDARDIZATION OF SLAGGING INDICES.
- EFFECT OF COAL PARTICLE SIZE ON SLAGGING/FOULING.

- o CORRELATION BETWEEN COAL-WATER MIXTURE ADDITIVE CHEMISTRY AND ATOMIZATION CHARACTERISTICS.

THE INCREASED EMPHASIS ON UNIVERSITY COAL RESEARCH IS STRONGLY ENDORSED. THIS CAN BE AN IMPORTANT ASSET SUPPORTING THE RESEARCH OPPORTUNITIES AND FREEDOM WHICH ULTIMATELY PROVIDE THE INNOVATIVE FOUNDATION FOR OUR NATIONAL SCIENTIFIC AND ENGINEERING CAPABILITY IN ENERGY.

## C. COMBUSTION SYSTEMS

### 1. ATMOSPHERIC FLUIDIZED BED COMBUSTION

AS EPRI INDICATED LAST YEAR, TERMINATION OF DOE SUPPORT FOR AFB TECHNOLOGY DEVELOPMENT IS OCCURRING AT A TIME WHEN THE TECHNOLOGY IS ACHIEVING A COMMERCIAL FOOTHOLD FOR INDUSTRIAL APPLICATION. THE DOE PROGRAM HAS BEEN INSTRUMENTAL IN AFB ACHIEVING THIS CAPABILITY TO RELIABLY PRODUCE STEAM FROM COAL FOR SMALL INDUSTRIAL AND SPACE HEATING APPLICATIONS. THE PROGRAM HAS INCLUDED INSTALLATION OF SEVERAL INDUSTRIAL DEMONSTRATIONS AND IS AN EXAMPLE OF THE SUCCESS THAT CAN BE ACHIEVED THROUGH A WELL PLANNED AND CONSISTENT GOVERNMENT RESEARCH, DEVELOPMENT AND DEMONSTRATION PROGRAM, CONDUCTED IN A MANNER COMPLEMENTING PRIVATE SECTOR CAPABILITIES AND OBJECTIVES.

EPRI AND THE UTILITY INDUSTRY ARE PURSUING THE APPLICATION OF AFBC AS A FURTHER EVOLUTIONARY IMPROVEMENT IN A COAL UTILIZATION TO MEET THE NEW REQUIREMENTS OF TODAY. THE IMPROVEMENTS WHICH EXCITE INTEREST INCLUDE REDUCED SENSITIVITY TO FUEL QUALITY, THUS PERMITTING USE OF A MUCH BROADER FUEL SUPPLY WITHOUT SUFFERING LARGE LOSSES IN EFFICIENCY AND RELIABILITY IN A SINGLE BOILER DESIGN. SECOND, AFB INDICATES LESS COST SENSITIVITY TO UNIT SIZE IN A PERIOD WHEN LOAD GROWTH AND SITING RESTRICTIONS MAY ENCOURAGE SMALLER RATHER THAN LARGER BOILERS. A THIRD PRIMARY ADVANTAGE OF AFB IS ENVIRONMENTAL PERFORMANCE. IN AFB BOTH SULFUR AND

NITROGEN OXIDE CONTROL IS ACHIEVED WITHIN THE COMBUSTION PROCESS, THUS ELIMINATING THE NEED FOR PARASITIC FLUE GAS CLEANING SYSTEMS.

DEVELOPMENT OF AFB HAS SUCCESSFULLY PROGRESSED FROM THE PROCESS CONFIRMATION STATE TO ENGINEERING PROTOTYPE, MAKING COMMERCIAL UTILITY SCALE APPLICATION A DISTINCT PROBABILITY THIS DECADE. TVA HAS TAKEN THE LEAD, WITH EPRI SUPPORT, IN IMPLEMENTING THE 20 $MW_E$ ENGINEERING PROTOTYPE WHICH BEGAN OPERATION IN MAY OF 1982. THIS FACILITY WAS ONLY POSSIBLE THROUGH MAJOR FEDERAL R&D SUPPORT.

BASED ON THE SUCCESS OF THIS TECHNOLOGY DEVELOPMENT, EPRI AND THE UTILITY INDUSTRY ARE MOVING AGGRESSIVELY TO IMPLEMENT COMMERCIAL SCALE (100-200 $MW_E$) DEMONSTRATIONS OF AFB. EPRI IS PRESENTLY EVALUATING PROPOSALS FROM FIVE SEPARATE UTILITIES REPRESENTING A WIDE RANGE OF COMPANY SIZES, INDUSTRY SECTORS AND GEOGRAPHICAL REGIONS. IN EACH CASE THE HOST UTILITY IS PROPOSING TO FUND THE LARGEST PORTION OF THE COST OF THE DEMONSTRATION WITH EPRI AND THE BOILER MANUFACTURERS SHARING THE FIRST-OF-A-KIND RISK COSTS WITH THE UTILITY. IN ADDITION, I BELIEVE THIS COMMITMENT WILL HELP MAINTAIN THE TECHNICAL AND COMMERCIAL LEADERSHIP OF OUR DOMESTIC BOILER MANUFACTURER SINCE THE CHARACTERISTICS THAT MAKE AFB ATTRACTIVE DOMESTICALLY HAVE EQUAL OR GREATER MERIT IN THE INTERNATIONAL MARKET.

THE CONTINUITY AND SUCCESS OF THE COMBINED FEDERAL/PRIVATE DEVELOPMENT EFFORT MAY MAKE THIS PIONEER DEMONSTRATION STEP FOR AFB COMMERCIALLY VIABLE FOR THE PRIVATE SECTOR. THIS IS, HOWEVER, LIKELY TO BE THE EXCEPTION RATHER THAN THE RULE UNLESS FEDERAL SUPPORT FOR HIGH RISK COAL COMBUSTION R&D, INCLUDING CRITICAL PILOT PLANTS, IS MAINTAINED AT A SUBSTANTIALLY GREATER LEVEL THAN REQUESTED IN FY 1984.

## 2. PRESSURIZED FLUIDIZED BED COMBUSTION

THE IMPACT OF THE PROPOSED DOE FUNDING REDUCTION IS NOWHERE MORE IMMEDIATELY EVIDENT THAN IN THE PFB DEVELOPMENT PROGRAM. MAJOR PROGRESS HAS BEEN MADE OVER THE PAST SEVERAL YEARS IN CONVERTING THIS TECHNOLOGY FROM AN ENGINEERING CURIOSITY TO AN IMPORTANT ELECTRIC POWER PRODUCING TECHNOLOGY HAVING THE LOWEST BUSBAR ENERGY COST POTENTIAL OF ANY NEAR TERM COAL UTILIZATION OPTION. THE EFFECTIVE CANCELLATION OF DOE SUPPORT ON THE BRINK OF SUCCESSFUL PROOF OF COMMERCIAL VIABILITY IS INDEED "SEIZING DEFEAT FROM THE JAWS OF VICTORY."

THE UNCERTAIN AND CONSTRAINED UTILITY MARKET ALSO INFLUENCES PRESSURIZED FLUID BED COMBUSTION (PFB) DEVELOPMENT GOALS. THE INFLUENCE ARISES FROM THE TREND TOWARD SMALLER UNIT SIZE AND GROWING UTILITY INTEREST IN UPRATING THE CAPACITY OF EXISTING UNITS TO BRING CAPACITY ON LINE AT THE LOWEST INVESTMENT COST. AS A RESULT, DEVELOPMENT EMPHASIS IS BEING PLACED ON TURBOCHARGED PFB BOILERS WHICH CAN PROVIDE SHOP-FABRICATED, BARGE TRANSPORTABLE STEAM GENERATION MODULES. THESE CAN BE RAPIDLY FIELD-ERECTED TO PROVIDE THE DESIRED UPRATING IN UNIT SIZES OF 150 TO 250 $MW_E$. THIS APPROACH WILL ALSO USE COAL TO REPLACE AND INCREASE THE CAPACITY OF EXISTING OIL- OR GAS-FIRED PLANTS WHILE MINIMIZING SPACE AND ENVIRONMENTAL CONTROL REQUIREMENTS. IN ADDITION, THE TURBOCHARGED BOILER USES A TECHNICALLY SIMPLER GAS TURBINE CONFIGURATION WHICH ONLY DRIVES THE BOILER COMPRESSOR AND IS NOT USED TO GENERATE ELECTRICITY.

THE PRIMARY PHYSICAL DIFFERENCE BETWEEN THE TURBOCHARGED BOILER AND THE PFB-COMBINED CYCLE THAT HAS BEEN PREVIOUSLY EMPHASIZED IS THE REDUCTION IN GAS TURBINE OPERATING TEMPERATURE. THIS SUBSTANTIALLY REDUCES THE

DEVELOPMENT RISK AND COST, AND IMPROVES THE RELIABILITY OF THE BOILER SYSTEM. BASED ON THE SUCCESSFUL NEAR-TERM INTRODUCTION OF THE TURBOCHARGED BOILER, THE TECHNICAL AND COMMERCIAL RISKS IN DEVELOPING THE HIGHER EFFICIENCY PFB COMBINED CYCLE TECHNOLOGY SHOULD ALSO BE SUBSTANTIALLY REDUCED.

EFFORTS ARE NOW BEING INITIATED BY EPRI TO DEVELOP ENGINEERING DESIGNS FOR THE PFB TURBOCHARGED BOILER BASED ON RESULTS FROM OPERATIONAL PILOT PLANTS. THIS APPROACH SHOULD PERMIT COMMERCIAL PROTOTYPES TO BE OPERATIONAL THIS DECADE, THUS YIELDING IMPORTANT FINANCIAL ADVANTAGES TO UTILITIES DURING THE CURRENT PERIOD OF LOW-LOAD GROWTH AND HIGH-CONSTRUCTION COSTS. THE COMBINATION OF SHORTER LEAD TIME AND SMALLER UNIT COST PROVIDED BY THE TURBOCHARGED BOILER MAY SUBSTANTIALLY REDUCE CAPITAL COSTS FOR GENERATIONS.

THE MAJOR LIMITING FACTOR RESTRICTING COMMERCIAL REALIZATION OF THIS CAPABILITY IS CONFIDENCE IN THE DESIGN BASE FOR THE PFB BOILER. ALTHOUGH MANY OF THE TECHNICAL ISSUES AFFECTING THE GAS TURBINE AND BALANCE OF PLANT HAVE BEEN REDUCED OR ELIMINATED BY THE TURBOCHARGED BOILER APPROACH, THE BOILER REMAINS THE CRITICAL PACING DEVELOPMENT ISSUE. UNDER THESE CONDITIONS I AM VERY CONCERNED BY THE PROPOSAL TO TERMINATE FEDERAL SUPPORT FOR THE INTERNATIONAL ENERGY AGENCY (IEA) GRIMETHORPE PILOT PFB FACILITY. I BELIEVE NO SINGLE ISSUE IS MORE CRITICAL TO THE COMMERCIAL SUCCESS OF PFB THAN CONTINUED OPERATION OF GRIMETHORPE. GRIMETHORPE IS THE ONLY OPERATIONAL PFB FACILITY IN THE WORLD WHICH HAS THE TECHNICAL CAPABILITY AND SIZE TO ADEQUATELY ADDRESS THE PACING DEVELOPMENT ISSUES. IT HAS BEEN THE CORNERSTONE FOR DEVELOPMENT OF PFB. IN ADDITION, IT IS SUPPORTED BY THE LARGEST CONCENTRATION OF PFB

EXPERIENCE IN THE WORLD. ITS RESULTS ARE NON-PROPRIETARY AND ARE AVAILABLE TO PARTICIPANTS IN EACH SUPPORTING NATION. IN SHORT, ITS CAPABILITIES ARE IRREPLACEABLE AND REPRESENT ONE OF THE MOST VALUABLE APPLICATIONS OF FEDERAL ENERGY R&D FUNDING.

AT THE PRESENT TIME GRIMETHORPE IS CONCENTRATING ON RESOLUTION OF IN-BED HEAT TRANSFER TUBE BANK EROSION. THIS IS OBVIOUSLY CRITICAL TO FURTHER PFB SYSTEM DEVELOPMENT. PREMATURE ELIMINATION OF U.S. SUPPORT AT THE END OF 1983 WILL CUT SHORT THIS EFFORT AND PREVENT THE LONG-TERM TESTING NECESSARY TO INSURE CONFIDENCE IN THE DESIGN BASE. FURTHER PFB DEVELOPMENT REQUIREMENTS WHICH NEED TO BE ADDRESSED AT GRIMETHORPE INCLUDE PROCESS OPERABILITY AND CONTROL, COAL AND ASH HANDLING, AND CONFIRMATION OF GAS CLEANUP SYSTEM RELIABILITY AND PERFORMANCE.

I THEREFORE STRONGLY RECOMMEND THAT U.S. SUPPORT A STREAMLINED GRIMETHORPE PROJECT TO BE CONTINUED FOR AT LEAST THE NEXT TWO YEARS. FAILURE TO DO SO WILL CUT US OFF FROM OUR MOST VALUABLE SOURCE OF DEVELOPMENT CAPABILITY AND STALL THE PRIVATE SECTOR'S EFFORTS TO ACHIEVE THE TECHNICAL AND COMMERCIAL ADVANTAGES OF PFB.

RESEARCH AND DEVELOPMENT GROUPS SUCH AS THOSE WHICH ADDRESS PFB ARE FRAGILE ENTITIES THAT ONCE DISBANDED CANNOT BE RESTORED. I WOULD THEREFORE URGE THAT REDUCTIONS OF THE LEVEL PROPOSED BE RECONSIDERED AND A MORE POSITIVE EFFORT BE MADE BY THE GOVERNMENT TO COORDINATE ITS PROGRAM PLANNING WITH THE PRIVATE SECTOR. WE HAVE LONG SUGGESTED A JOINT EFFORT WITH DOE TO POSTURE A COMPLEMENTARY R&D PLAN IN PFB THAT BEST USES THE LIMITED FEDERAL AND PRIVATE RESOURCES. IN SPITE OF THE MAJOR REDUCTIONS PROPOSED BY DOE, NO MEANINGFUL PRIOR COORDINATION HAS BEN INITIATED WITH THE PRIVATE SECTOR.

BASED ON THE SUCCESS OF GRIMETHORPE AND OTHER PFB DEVELOPMENT PROGRAMS, EPRI PLANS TO IMPLEMENT A COMMERCIAL TURBOCHARGED PFB PROTOTYPE WITH THE UTILITY INDUSTRY THIS DECADE. ABSENCE OF GOVERNMENT FUNDING INCREASES THE NECESSITY THAT SUCH AN INSTALLATION PROVIDE ACTUAL ELECTRIC GENERATING CAPACITY TO INTEREST THE NECESSARY PRIVATE SECTOR FUNDING. ALTHOUGH THE EPRI PLAN CONTAINS SUPPORT FOR SUCH A PROJECT, ITS FULL FUNDING MECHANISM IS NOT YET EVIDENT. I ALSO EMPHASIZE THAT SUCH A COMMERCIAL DEMONSTRATION WILL PROBABLY ONLY BE IMPLEMENTED IF THE GRIMETHORPE RESULTS INDICATE, TO USERS SATISFACTION, CONFIDENCE IN THE RELIABILITY AND OPERAABILITY OF PFB POWER PLANTS.

## CONCLUSIONS

1. COAL COMBUSTION RELATED TECHNOLOGY WHERE CONTINUED FEDERAL R&D PARTICIPATION IS MOST CRITICAL MUST START WITH PFB. THE PROPOSED TERMINATION OF THE GRIMETHORPE PILOT FACILITY MAY STALL THE PROMISING PFB TECHNOLOGY INITIATIVE UNDERWAY IN THE PRIVATE SECTOR TO PROVIDE A MODULAR, LOW COST, CLEAN AND MORE RAPIDLY BUILT POWER GENERATING CAPABILITY THIS DECADE.

2. GREATER FEDERAL EMPHASIS SHOULD BE PLACED ON RESOLVING THE TECHNICAL ISSUES LIMITING THE EFFECTIVENESS OF MANDATED ENVIRONMENTAL CONTROL TECHNOLOGY. THESE LIMITATIONS, COMBINED WITH UNCERTAINTY OVER THE FUTURE COURSE OF REGULATORY REQUIREMENTS, POSE ONE OF THE GREATEST IMPEDIMENTS TO INCREASED COAL USE.

3. THE ALTERNATIVE FUEL UTILIZATION PROGRAM SPONSORED BY DOE IS ESSENTIAL, PARTICULARLY IN THE AREA OF COAL-WATER MIXTURES. THIS WORK, TOGETHER WITH THE EFFORTS SPONSORED BY EPRI AND OTHERS IN THE PRIVATE SECTOR, WILL PROVIDE THE NECESSARY TECHNICAL CONFIDENCE FOR COMMERCIALIZATION OF THIS IMPORTANT OIL REPLACEMENT OPTION. DEVELOPMENT OF TECHNIQUES TO PRODUCE STABLE, PUMPABLE MIXTURES OF COAL AND WATER OFFER THE

POSSIBILITY OF A SUBSTANTIAL FUEL COST SAVING THAT COULD JUSTIFY CONVERSION OF OIL-DESIGN BOILERS TO COAL

4. LONGER RANGE, HIGHER RISK COAL PROGRAMS WHICH SHOULD ALSO RECEIVE HIGHER PRIORITY FOR CONTINUED FEDERAL R&D SUPPORT ARE MATERIALS RESEARCH, DIRECT COAL UTILIZATION, AND COAL PREPARATION AND ANALYSIS. OF PARTICULAR IMPORTANCE IN THE LATTER CATEGORY IS HIGHER CONFIDENCE IN OUR COAL RESERVE QUALITY AND QUANTITY ESTIMATES AND IMPROVED QUALITY CONTROL TECHNIQUES FOR OUR LOWER RANK COALS.

   THE REDUCTION IN EMPHASIS ON DIRECT COAL UTILIZATION IS ALSO OF MAJOR CONCERN. IF THIS REDUCTION IS CONFIRMED, IT WILL HAVE A MAJOR IMPACT ON THE NATIONAL TECHNOLOGY BASE NECESSARY TO MAINTAIN OUR COMPETITIVENESS IN BOTH DOMESTIC AND INTERNATIONAL MARKETS.

5. THERE IS NO SHORTAGE OF OPPORTUNITIES TO IMPROVE THE EFFICIENCY, RELIABILITY AND ENVIRONMENTAL PERFORMANCE OF COAL COMBUSTION. THE SUCCESSFUL ACHIEVEMENT OF THE OBJECTIVES LAID OUT IN THIS TESTIMONY REQUIRES, HOWEVER, A MORE CONSTRUCTIVE AND COORDINATED PARTNERSHIP BETWEEN GOVERNMENT AND THE PRIVATE SECTOR. THIS ENCOMPASSES THE PLANNING AND IMPLEMENTATION OF R&D AS WELL AS THE COMMERCIAL TRANSFER OF RESULTS.

6. THE RATE OF COMMERCIALIZATION AND USE OF IMPROVED TECHNOLOGY ALSO DEPENDS ON THE DEVELOPMENT OF REGULATORY AND ECONOMIC INCENTIVES WHICH ENCOURAGE INTRODUCTION OF INNOVATIVE TECHNOLOGY. TOO OFTEN OUR DEPENDENCE ON THE ADVERSARIAL APPROACH TO RESOLUTION MAKES THE CONFLICTS RESTRICTING COAL USE MORE SEVERE AND DISRUPTIVE THAN NECESSARY AND RESTRICTS THE INTRODUCTION OF TECHNOLOGICAL IMPROVEMENT.

Mr. **Durbin**. Thank you very much.

I would like to ask a few preliminary questions, and you gentlemen can decide whose jurisdiction is imperiled by these questions and apportion them accordingly.

First off, just by way of background, what is the projected contribution or amount of money to be spent by your institute, EPRI, in the next year in comparison to Federal spending?

Mr. **Spencer**. EPRI's total research and development budget is about $250 million in this calendar year, and that covers all of our coal technology, nuclear, electrical systems, fuel cells, et cetera.

Mr. **Durbin**. How has that changed, let us say, in the last 2 or 3 years?

Mr. **Spencer**. We have been relatively constant for the last 2 years in funding. In the last 5 years, we have grown approximately 10 percent per annum.

Mr. **Durbin**. Can either or both of you comment on any experiences you have had in using or working with the national laboratories? As you are aware, I am sure, we are interesting in assessing their role in the whole energy picture from a national perspective, and we are interested in determining whether EPRI has utilized these laboratories, and their opinion of them.

Mr. **Spencer**. I will answer, Mr. Chairman, in the context of our coal technology development efforts, that the national laboratories have been a minimal source of technical capability, because they have a much stronger background, obviously, in the nuclear industry.

However, we have on occasion certainly utilized some of the Morgantown Research Center capabilities in coal gasification. We have had interactions with the Grand Forks Energy Technology Center and a number of the other energy technology centers on a technical exchange basis, not as much from a joint project funding basis.

Mr. **Durbin**. I would like to ask you a little bit about your testimony, Mr. Spencer, as it relates to coal liquefaction. I found it interesting that you are one of the first witnesses who has ventured into the area of telling us how much you anticipate the barrel of distillate will cost in years to come and how economies of scale might be applied to it, and that, of course, in the context of present OPEC pricing is something we are all very conscious of.

Do I infer from what you have said this morning that you anticipate some economies of scale by large production that you cannot see at the present time which might bring this distillate product into a more competitive position?

Mr. **Spencer**. I would say there are really probably three factors that are most important. First, we do see some process improvements that have now become evident to us from our small pilot plant scale that show promise of perhaps reducing, let us say, that $55 a barrel price down to perhaps $45 per barrel. But I cannot see any process improvements that are likely to take it beyond that. About a 20 percent improvement in price is the best we can see, even at, let us say, a 3,000- to 5,000-tons-per-day scale module.

We really believe it is going to take some type of a subsidy program to get the initial commitments to pioneer plant facilities so that we have a firm basis for commercial plants in the future. That subsidy form, I think, may have to be both a direct outlay consideration by the government as well as some form of tax incentives that would enhance the interests of, let us say, primarily the petroleum industry in investing in such plants.

The electric power industry would not be investing in these types of plants. We would anticipate it would primarily be the petroleum-related industry.

Mr. Durbin. You made one comment in here about expensing construction costs and highly leveraged financing offering a further reduction. Would that be beyond the $45 per barrel figure?

Mr. Spencer. Yes, that would. That would then impact by another 20 to 33 percent beyond that, so that that would get the price almost comparable to what we would anticipate world oil prices to be.

Mr. Durbin. How far away would we be from that happening if we had a commitment from the Federal Government to make that kind of subsidy expenditure and to get involved in the coal liquefaction process in a big way?

Mr. Spencer. Well, if such a commitment were made in 1983, we could have certainly two demonstration plants at the 3,000 to 5,000 tons per day scale on line no later than 1990 and perhaps a little earlier than that, but certainly by the end of this decade.

Mr. Durbin. Do you have any idea in your own mind what kind of dollar commitment we are talking about to achieve that?

Mr. Spencer. Typically, the investment cost for a pioneer plant would be approximately $100,000 per daily barrel of capacity, so that if you took, let us say, a 5,000-ton-per-day facility and $100,000 a daily barrel, that says we would have about a $3 billion total investment commitment for two of those plants.

Mr. Durbin. What I found curious in the testimony today and yesterday was the continuing suggestion that we look to the Department of Energy for assistance in many of these areas. I have often wondered what role the SFC is playing in this, or should be playing. Could you comment on that?

Mr. Spencer. I would prefer not to comment on the role they should be playing. I would say that, as I have indicated in the testimony, I believe that there are initiatives on the part of the Government beyond those that are presently in existence that will be necessary to bring the proper organizations and proper technologies to demonstration scale.

So I would say that the SFC has a role to play, but at least in my judgment, in the case of coal liquefaction, it will not be sufficient in order to bring that technology to a full realization.

Mr. Schwolsky. Mr. Durbin, I would like to amplify on that.

The Synthetic Fuels Corporation does not have jurisdiction over areas such as pressurized fluidized bed combustion as well as atmospheric fluidized bed combustion. Also, there really are not that many economic incentives available to the utility industry with respect to tax breaks to encourage the progress of these technologies.

Mr. Durbin. Have there been any suggestions by way of legislation that you are aware of that would change that situation?

Mr. Schwolsky. I know there has been talk with respect to giving the utility industry certain tax breaks, but I don't believe there is anything that is firmly along the way.

Mr. **Durbin**. I guess we would call into mind here a really fundamental question of philosophy, and that is whether we can expect, if we gave your industry those kinds of tax incentives, that they would carry the ball in terms of future research. I think the administration is of the opinion, and Secretary Hodel has said from time to time, that cutbacks in the DOE budget, for example, are being made with the anticipation that private industry will step in.

Do you see, with a tax incentive program, a larger role by private industry in developing some of these technologies?

Mr. **Schwolsky**. I certainly would hope that there would be a larger role by the private sector to take up some of the gap. I obviously cannot guarantee it, but I would imagine there would probably be some effort on their part to move ahead a little more aggressively with regard to the development of these technologies.

Mr. **Durbin**. It would appear, using the old "no free lunch" philosophy, that if we are not going to spend the money through DOE, we are going to spend it indirectly through tax credits or tax benefits to the industry to encourage them to move in this area.

Mr. **Schwolsky**. Like the administration, I would prefer to see the private sector managing this effort as opposed to the Department of Energy.

Mr. **Durbin**. Would you feel that if we had a generalized tax credit type of approach we could expect the industry to specify those projects that would have the most promise and move into a balanced approach kind of research?

Mr. **Schwolsky**. Yes; I think that certainly the industry knows their particular needs, perhaps much better than the Government, so I would expect them to earmark those particular technologies which are going to have the greatest impact on a near-term basis.

Mr. **Durbin**. On a near-term basis?

Mr. **Schwolsky**. Near-term basis meaning over the next 10 years.

Mr. **Durbin**. I think perhaps that is where we will have a departure in what our role is and should be and what you see from your industry perspective.

Mr. Spencer, I am particularly interested in the area of your testimony relating to methanol and the liquid-phase methanol process.

I live in an area that not only has coal but a great deal of agricultural bounty and the production in Decatur, Ill., of substantial amounts of ethanol.

I will put you to a test again here. Can you give me some ideas, if you will, of what the cost is to produce methanol from coal?

Mr. **Spencer**. Yes, Mr. Chairman, we have actually looked at production of methanol in two different ways, one with a dedicated plant per se producing just methanol as a product and with normal, 100 percent equity financing. In that case, the cost of the methanol, in 1982 dollars, we estimate to be between $8 and $12 per million Btu's. If we combine the methanol production with electricity from coal in an integrated gasification combined-cycle plant, the cost then would be approximately $6 per million Btu's or, if you multiply that times about 6 million Btu's per barrel, very comparable to today's high quality distillate prices.

In the latter case, however, where it is integrated with gasification combined-cycle plant, the product would be about 15 percent methanol, about 85 percent electricity, and that is why the economy in the cost of methanol.

Mr. DURBIN. We were trying to do some quick computations.

Mr. SPENCER. All right. I can provide your staff with whatever additional information you might like in that regard.

Mr. DURBIN. I would appreciate that very much.

There is a lot of discussion in your testimony about the Wilsonville facility. In the event that there was no Federal funding of this facility, what do you think would be the future of it in the near term and long term?

Mr. SPENCER. I do not think there would be a long-term future for it.

In the near term, if the Federal funding is not available, I would suspect that EPRI would phase the facility out in no more than 6 months from now, because we just are not in a position to make a larger commitment than the $3.5 million a year that we are presently providing for it, so that I think it would be a major loss, from my standpoint, of a national asset, but I do not see a way that we would be able to carry the funding for the plant ourselves.

Mr. DURBIN. One of the things that we are curious about as well is your evaluation of the current technical status of direct coal liquefaction, particularly whether you think it is appropriate at this point to go forward with a 5,000 to 10,000 barrels per day plant such as the SFC might be persuaded to support.

Mr. SPENCER. I do think it is very important to proceed within the next year or two because the technology base is now nearly in hand. We have still the organizational capability that could be mobilized within the two or three organizations that have this technology understanding. Unless we act in the next year or two, that demobilization will essentially prevent a rapid utilization of that technology in the future.

So, from a contingent liability standpoint or a contingent development standpoint, as I think I indicated in my testimony, I do believe that the country should commit to a couple of pioneer plants in coal liquefaction.

Mr. DURBIN. Mr. Schwolsky, one portion of your testimony that you read of Mr. Yeager which I found interesting was on page 5, relative to the penetration of these advanced technologies into utility generation. You have indicated—at least Dr. Yeager has indicated—a target date of 1990. Is that an indication that the technologies we are discussing in his paper might be of practical use to us in electric generation only by 1990?

Mr. SCHWOLSKY. Certainly, with regard to atmospheric fluidized bed combustion, we would hope that we could begin construction by 1984 with a 100- to 200-megawatt commercial demonstration of that particular technology, with perhaps introduction to utility application by 1988, 1989.

With regard to pressurized fluidized bed combustion, if in fact we go with the turbocharged PFBC steam generator powered by a gas turbine, and if in fact Grimethorpe is continued and consequently provides us with the technical information to move forward with respect to the turbocharged approach, we would be hopeful that perhaps we could have commercial demonstration of that program also by 1987, 1988, and introduction to the utility market probably in 1989.

**Mr. Durbin.** I guess I will exercise my prerogative by having arrived first and having the gavel in my hand.

Which do you think is the most promising technology as it relates to No. 6 coal? I think either one or both of you had indicated that there was some research relative to the use of that coal.

**Mr. Schwolsky.** I will defer to Mr. Spencer on that.

**Mr. Spencer.** I think we really have two good alternatives that are under development now, the atmospheric fluidized bed combuster, and that is a technology that is an extrapolation of present experience in the utility industry. I believe that technology shows a lot of promise for early utilization of high-sulfur coals.

If the acid rain legislation which is being considered by the Congress is indeed imposed on the utility industry and we have to meet even tighter emission control requirements in the future, then I am personally convinced that gasification combined cycle technology will indeed offer the most attractive option for utilizing high-sulfur eastern coal, that we can essentially remove 97 to 99 percent of the sulfur from the coal in the process, and essentially the emissions from such plants are extremely minimal; yet at the same time we are able to obtain overall efficiencies and costs competitive with coal-fired plants.

So, in the long term, I believe coal gasification combined cycles will clearly be the choice. In the interim, I think both the combined cycles and atmospheric fluidized bed combustion options are very important.

**Mr. Durbin.** How close is either of these technologies to commercialization?

**Mr. Spencer.** Well, the demonstration plant for cool water will be complete next year. If we get good operating data, say between 1984 and 1986, the first commercial commitments for that type of technology could be made in 1986. There is normally a 7-year time period from licensing through construction, so by 1992 to 1993, the first commercial plants could be on line.

AFBC would be, nominally, 2 to 3 years behind that, but approximately the same timeframe, the midnineties time period.

**Mr. Durbin.** Would this development of the cool water gasification process require an increased commitment over what the Federal budget presently calls for to see us meet that target date?

**Mr. Spencer.** No. We have in this case completely capitalized that project with private sector funds, and now with the Synthetic Fuels Corporation price guarantee, I think we now have the assurance we need to be able to operate for the 5 years to obtain the type of data that we feel is necessary for a commercial plant basis.

**Mr. Durbin.** Do you have an opinion as to what the impact on price is of this energy production, by going to this gasification project? Can you give me an idea what it is going to cost and add to the process of generating Btu's?

**Mr. Spencer.** Well, in this case, since we actually are taking coal to electricity, we look at the bus bar cost of electricity, and we estimate in 1982 dollars for integrated gasification combined cycle, in fact for a plant that would be located near Springfield, Ill., of all places—that happens to be the location we select for our analysis purposes, amazingly enough——

Mr. **Durbin**. My ears perked up there for a moment.

Mr. **Spencer** [continuing]. The cost estimates are about 5 cents a kilowatt-hour, between 5 and 5½ cents a kilowatt-hour, basis coal. If you were going to utilize a coal-fired plant with scrubbers, it would be 5½ to 6 cents.

So we believe we are within perhaps 10 percent less in cost than a coal-fired plant with scrubbers with that same Illinois No. 6 coal.

Mr. **Durbin**. Taking you out of your area for a moment here, at least your testimony, how would that compare to a nuclear-powered facility?

Mr. **Spencer**. It is a little difficult to say at this point in time. I would rather not give you an estimate of nuclear power in that same area because I am just not that familiar with the present costs of nuclear plant construction, and I feel much more comfortable in talking about the technologies that I am more directly familiar with.

Mr. **Durbin**. I would like to ask you one last question before I open it up to other members who might have questions. This is on the Grimethorpe project. The question that I have at this point is this: In Dr. Yeager's testimony, there is an indication that he would strongly recommend that the United States support a streamlined Grimethorpe project. Can you put a price tag on that type of commitment?

Mr. **Schwolsky**. I think, insofar as total funding, with respect to not only the United States participating but perhaps the British and the West Germans and maybe even some other countries, we are looking at a figure between $15 and $30 million a year. Now, how that breaks down amongst the three existing participants and perhaps more, I really cannot say at this time.

Mr. **Durbin**. That is all the questions I have.

Mr. Bateman.

Mr. **Bateman**. Thank you, Mr. Chairman.

I apologize for coming in late and not hearing a good deal of your testimony. I am just in the process of running quickly through the statements.

I have a rather general question I would like to frame and see if you could give me any help with it. If you are going to obtain energy in a liquid form from the use of coal as the beginning material, do you go through, in all instances, under everyone's technology, a gasification process before you ultimately come to liquefaction?

Mr. **Spencer**. No, Congressman Bateman.

There are direct hydroliquefaction process technologies, as we call them, and that is the Exxon donor solvent and H-coal process and the Wilsonville process, all of which are mentioned in the testimony. In these cases, what we do is we take part of the oil that we produce from the coal; we recycle that back and actually dissolve the coal in part of that oil, and then it is maintained in an oil state throughout the process.

We then have to, obviously, remove the ash from the coal, from that oil, through various distillation processes, but in that case it is all in either a solid or liquid State, rather than having to go to gasification first.

**Mr. Bateman.** All right.

**Mr. Spencer.** For methanol synthesis, we would go through a gasification process and then a subsequent catalytic synthesis to produce methanol.

**Mr. Bateman.** So, under at least one set of technological approaches, you never go through the gasification in order to arrive at the liquefaction.

**Mr. Spencer.** That is right.

**Mr. Bateman.** I take it there is another technological approach whereby you do go from coal to a gasified state to a liquefied state.

**Mr. Spencer.** That is correct. In fact, the SASOL plant in South Africa utilizes that process technology.

**Mr. Bateman.** Well, have those in the scientific community who have devoted great time and attention to this arrived at any consensus as to which is the most cost-effective way to go?

**Mr. Spencer.** That is an excellent question.

I would say, within our ability to estimate the price of liquid fuels from coal, that either direct hydroliquefaction processes or indirect synthesis by going through gasification and then producing liquids, for a high quality fuel, let us say, a very light distillate fuel, the price would probably be comparable.

However, to produce boiler or gas turbine fuels from coal, we believe that direct hydroliquefaction would be approximately 10 to 15 percent less expensive. But there is still an uncertainty in estimating those costs at this time.

**Mr. Bateman.** Well, looking at the mix of energy needs and the things that you use energy to accomplish, do we need to put greater emphasis on the kind of liquefied end products that would be used for generating electricity, or do we need the more refined product? What do the economics tell us as to where the emphasis ought to go on the product that is utilized from the conversion of coal to other forms of energy?

**Mr. Spencer.** Let me state that my view is that by utilizing direct hydroliquefaction without going through the gasification step, we are able to produce a broad range of products from very light distillates to gas turbine and boiler fuels, and so, therefore, we can price those fuels which are of poorer quality to the utility industry at a price that is more attractive to them, and those that would compete directly with gasoline feedstocks can still compete in that marketplace, still with subsidy at this point. They both really require subsidy at this point in time.

But the key thing is that we do not have the large-scale experience with the direct hydroliquefaction processes. We have run two large pilot plants. We need now to take a step to about 3,000 to 5,000 tons per day of throughput in order to achieve and demonstrate the commercial potential of those processes.

In the case of indirect synthesis, we have built gasification reactors in the 800 to 1,000 tons per day scale. Those are then modularized to produce the large plants like SASOL, so there is not as much uncertainty in the process technology for indirect synthesis.

**Mr. Bateman.** One of the things that concerns me, though I by no means have arrived at any conclusion on any of this, is that if the South Africans have demonstrated with their SASOL plant that the technology is there, that the technology, at least for them, works economically. Aren't we at a point where, when the cost of the end product dictates doing it, we could move rather rapidly to meeting energy needs through that means and through commercial ventures as opposed to the devotion of continued large sums of public monies?

In other words, isn't there a technological base that is already in place and has been in place, at least in South Africa, for some years? We have gone through pilot plants here that demonstrate that the technology apparently works. When do we reach the point where this is a private sector concern which they can and should meet?

Mr. Spencer. That is a very difficult question, but let me try to address it in two ways.

First, I think we reach the point where the private sector assumes the responsibility for commercializing the technology when it is clear that the product prices from such technologies can compete with natural gas and oil. Until we achieve that point in time, the private sector just is not going to be prepared to make those major investments.

Mr. Bateman. Until that point, however, why should public funds be used to generate a product or to develop a product that is more expensive than is otherwise available, at this point in time?

Now, all of us have a concern that what is the state of facts at this point in time is not always going to remain so, and we are going to have to phase ourselves in to these new capabilities. But isn't the technology there, and the only thing that is absent is the present economic dimension of supply versus price of orthodox types of fuel?

Mr. Spencer. The Lurigi technology that is utilized in SASOL is a commercially available process technology, as well as the synthoil synthesis plant for diesel fuel.

However, normally, that would not be the primary product slate that one would want to have in the United States. You would rather have a product slate that has naphtha, gas turbine, and boiler fuels all as products.

So the SASOL technology is not the best fit to the end uses in this country, No. 1. Second, I believe that we need a variety of process technologies so we do not have to turn to a single process developer for the development of, let us say, a broad commercial capability in this country. I would hate to see us rely strictly on, let us say, Lurgi technology. That is the basis for the SNG plant that is being built, as you are probably aware, in North Dakota by ANR, but I believe that we ought to have domestic-developed process technologies like the Exxon donor solvent and H-coal processes that provide us, then, a variety of ways in which to produce crude oil from coal so that we have a competitive environment amongst the synthetic fuels.

I am very concerned, as I testified, in fact, a couple of years ago, that if we rely strictly on the technologies that are presently developed, when we get there, we will not have a basis for competitive pricing among various synthetic fuels processes. I think that is just as important as the ability to show that those synthetic fuels can compete with oil and gas.

Mr. **Bateman**. Mr. Chairman, that is all I have for now.

Mr. **Durbin**. I want to thank you, Herb. I want to tell you that some of the comments you are making about South Africa sound like my campaign speeches from the last election campaign. [Laughter.]

Gentlemen, I want to thank you very much for coming in today.

We have been joined here by Chairman Fuqua, who will take over for the next panel. I appreciate your coming before us. Thank you.

Mr. **Fuqua**. I want to thank the first panel. I apologize for not being able to be here, but, unfortunately, with all of our technology, we have not been able to develop methods whereby we can be in two places at one time. We try.

Our second panel is Dr. Charles E. Kolb, vice president of the Applied Research Division of Aerodyne Research, Inc., in Massachusetts; Dr. Eric Leber, American Public Power Association, here in Washington, and Mr. Malcolm Jones, Ebasco Services, from California.

If any of you gentlemen have statements and wish to summarize them, we will make the prepared statement part of the record. If you wish to summarize, we would be pleased to receive it in that fashion.

Dr. Kolb, we are pleased to have you. Would you please proceed.

### STATEMENT OF DR. CHARLES E. KOLB, VICE PRESIDENT, AND DIRECTOR OF THE APPLIED RESEARCH DIVISION, AERODYNE RESEARCH, INC.

Dr. **Kolb**. Mr. Chairman and members of the committee, thank you for inviting me to testify about proposed funding levels for the Department of Energy's fiscal year 1984 fossil energy programs. I appreciate the opportunity to comment on the level and direction of the very serious reductions in proposed Federal support for research and development in fossil energy.

While I hope to address the fossil energy research program from a general point of view, it is fair to note that my major perspective is that of director of Applied Sciences Division at Aerodyne Research, Inc., a small high technology business performing contract research and development for Government and industrial programs.

For the past 7 years, under ERDA and DOE funding, we have been engaged in studies designed to improve combustion efficiency, reduce hot gas corrosion, and improve the exhaust pollutant characteristics of advanced coal burning technologies. We have a demonstrated and published record of fundamental contributions to the basic phenomena underpinning technologies such as fluidized bed coal combustors, coal gasifiers, coal combustion gas-fired turbines, and coal-fired magnetohydrodynamic generators.

We currently participate in the fossil energy's coal fuel gas and gas stream cleanup programs under contract to the Morgantown Energy Technology Center, and the advanced research and technology development program through the Oak Ridge National Laboratory.

My personal perspective also includes the problems of university-based research, particularly at Harvard University and the Massachusetts Institute of Technology, where I have current research staff appointments and participate in a number of joint programs.

The proposed fiscal year 1984 appropriation of $94 million for the fossil energy research and development program represents a 57-percent reduction in appropriations from the fiscal year 1983 budget and over a 77-percent reduction from the fiscal year 1982 budget.

There are three possible justifications for budget reductions of this magnitude: One, the United States has solved most of its fossil energy supply problems in the past 2 years; two, the perceived fossil fuel supply problem was drastically overstated in 1982, leading to a serious overinvestment in fossil energy research and development; or, three, we have found someone other than the Federal Government who is going to solve the problem for us, thus relieving the Federal Government of responsibility to fund and administer a sustained and rational fossil energy R&D program.

Unfortunately, I find it hard to believe that any of the three possibilities listed above contains any strong component of the truth. While it is true that gasoline and heating oil are temporarily somewhat less expensive than last year, it is also true that there has been no fundamental change in the finite nature of recoverable petroleum resources and that coal has not suddenly become easier, cheaper, or safer to mine, transport, burn, or transform in ways that are environmentally and economically acceptable.

Thus, I must conclude that the long-term fossil energy supply problem is just as serious as it was in 1982, and, in my estimation, that is very serious indeed.

Furthermore, the quaint notion that the private sector will respond in a timely fashion with new technology to solve the problem on its own is clearly untenable in the face of the number of oil shale and coal gasification and liquefaction programs abandoned by industry during the past year. This prospect is also dimmed by the snail's pace of major projects associated with the Synthetic Fuels Corporation.

Furthermore, the generally poor economic climate has depressed an already inadequate level of industrially sponsored fossil energy research and development.

The sole bright spot in the area of non-Federal support is the ongoing research agenda sponsored by industrial associations such as the Electric Power Research Institute and the Gas Research Institute. The programs sponsored by these industrial groups are important and welcome, but they are no substitute for a comprehensive federally supported program.

In short, the long-term fossil energy problem has not been overstated, solved, or successfully transferred to someone else for solution. Thus, in my opinion, it is incumbent on the Department of Energy to mount a credible baseline research and technology development program for fossil energy supply and utilization problems. I seriously doubt that the proposed budget will allow DOE to achieve this goal, and I would like to indicate at least a few specific areas where they will fail.

While DOE has been guilty of wasting funds in the past, principally by rushing prematurely into expensive large-scale demonstration projects, it can be proud of stimulating the development of a skilled pool of scientists and engineers versed in fossil energy problems and skilled in the techniques required for their solution.

This pool of technical talent was woefully shallow when the energy crisis emerged a decade ago. DOE funding for university, industry, and government laboratories has since stimulated the development of significant numbers of skilled people and the concomitant investment in laboratories specifically designed for fossil energy research.

At the very least, DOE should be sponsoring a fossil energy R&D program which insures the continued existence of those superior university, industrial, and governmental laboratories with demonstrated records of achievement in fossil energy research. The DOE program should also promote the continued training and equipping of superior scientists dedicated to solving key fossil energy programs.

The proposed fiscal year 1984 budget, coupled with last year's reductions, clearly informs this pool of technical talent—and the organizations which recruited them and equipped their laboratories—that the whole thing was a mistake. With this DOE budget proposal, the current administration clearly signals its determination to dismantle the hard-won competence achieved over the past 10 years.

This message is further amplified by continued reductions in allied programs by the Environmental Protection Agency and the National Bureau of Standards.

In addition to generally threatening the national level of competence to deal with present and future fossil energy problems, the proposed budget eliminates or seriously reduces efforts in many specific areas of current concern. I find many of these reductions particularly hard to justify, and I would like to highlight just a few.

First, in the area of control technology and coal preparation for direct coal utilization, the proposed budget for gas stream cleanup is cut 55 percent from fiscal year 1983, and the program on cleanup base technology is eliminated entirely.

The gas stream cleanup program is aimed at removing noxious and/or corrosive species from the combustion streams of advanced coal utilization devices such as fuel cells or advanced gas turbine combined cycles. Such devices offer the best long-term hope for both the efficient and economic conversion of coal to electricity and the solution of environmental problems, such as acid rain, caused by the emission of sulfur and nitrogen oxides, or health effects due to emission of heavy metal containing particulates.

Many important basic questions involving homogeneous chemical and physical transformations, heterogeneous chemistry, and mass transport remain unanswered. With the budget more that halved, basic knowledge needed for the design and analysis of advanced systems will not be gained, thus further discouraging development efforts by utilities and their equipment vendors.

The cleanup base technology program was designed to help characterize, decontaminate, volume reduce, and dispose of the condensed phase coal preparation and coal combustion waste streams which are created by efforts to reduce sulfur oxide emissions from large coal combustion sources. This technology is urgently needed so that economic and environmentally sound methods of sulfur oxide reduction can be achieved. Need for these sulfur oxide reduction methods will be acute if current links of sulfur oxide emissions to serious health problems and/or serious acid rain effects are sustained. Elimination of all work on this portion of the sulfur oxide removal problem is extremely short sighted.

Second, the advanced research and technology development program for coal is designed to provide the baseline research, for which even the current administration agrees it cannot expect the private sector to pay. Nevertheless, this program is budgeted to suffer a 15-percent cut from fiscal year 1983 levels and a 45-percent cut from fiscal year 1982.

Areas unadvisedly cut include the materials program, which is crucial and generic to the development of more efficient and environmentally acceptable advanced combustion, gasification, and liquefaction systems.

The environmental activities program is scheduled for halving in the face of increasing international concern over topics such as acid rain.

The instrumentation, control, and diagnostics program is zeroed out despite the recognition that recent advances in instrumentally controlled combustion offer exciting advances in environmentally acceptable direct coal combustion.

Third, the magnetohydrodynamics program, which offers the potential of very efficient coal to electricity conversion with negligible sulfur oxide emissions, clearly grew too big too fast. However, the proposed complete abandonment of a program this promising makes no sense. At the very least, a low level MHD technology base program should be funded to maintain, consolidate, and analyze the valuable data gained in the program to date.

I have highlighted unwise reductions in programs in which my colleagues at Aerodyne and I have direct experience. I have no doubt that similarly unwise reductions for fiscal year 1984 are contained in other portions of the fossil energy research and development programs.

In summary, I believe the severe budget reductions proposed for the fiscal year 1984 fossil energy R&D program are unwarranted and unwise. If implemented, they threaten specific program goals which are still as important as ever if we wish to develop a safe, clean, economic, and assured supply of fossil energy.

Furthermore, the proposed budget clearly threatens the continued existence of a stable, well trained, and well equipped pool of scientists and engineers capable of dealing with present and future fossil energy programs.

Thank you.

[The prepared statement of Mr. Kolb follows:]

TESTIMONY OF DR. CHARLES E. KOLB
OF
AERODYNE RESEARCH, INC.
BILLERICA, MA 01821

TO THE
SUBCOMMITTEE ON ENERGY DEVELOPMENT AND APPLICATIONS
OF THE
COMMITTEE ON SCIENCE AND TECHNOLOGY
U.S. HOUSE OF REPRESENTATIVES

March 17, 1983

Mr. Chairman and Members of the Committee:

Thank you for inviting me to testify about proposed funding levels for the Department of Energy's FY '84 Fossil Energy programs. I appreciate the opportunity to comment on the level and direction of the very serious reductions in proposed federal support for research and development in Fossil Energy research and development.

## Background

While I hope to address the Fossil Energy Research program from a general point of view, it is fair to note that my major perspective is that of Director of the Applied Sciences Division at Aerodyne Research, Inc., a small high technology business performing contract research and development for government and industrial programs.

For the past seven years, under ERDA and DOE funding, we have been engaged in studies designed to improve combustion efficiency, reduce hot gas corrosion, and improve the exhaust pollutant characteristics of advanced coal burning technologies. We have a demonstrated and published record of fundamental contributions to the basic phenomena underpinning technologies such as fluidized bed coal combustors, coal gasifiers, coal combustion gas fired turbines, and coal fired magnetohydrodynamic generators. We currently participate in the Fossil Energy's Coal Fuel Gas and Gas Stream Cleanup Programs under contract to the Morgantown Energy Technology Center, and the Advanced Research and Technology Development Program through the Oak Ridge National Laboratory.

My personal perspective also includes the problems of university based research, particularly at Harvard University and the Massachusetts Institute of Technology, where I have current research staff appointments and participate in a number of joint programs.

## Problem Analysis

The proposed FY '84 appropriation of $94M for the Fossil Energy Research and Development Program represents a 57% reduction in appropriations from the

FY '83 budget and over a 77% reduction from the FY '82 budget. There are three possible justifications for budget reductions of that magnitude: 1) the United States has solved most of its fossil fuel supply problems in the past two years; or 2) the perceived fossil fuel supply problem was drastically overstated in 1982, leading to a serious over investment in fossil energy research and development; or 3) we have found someone other than the federal government who is going to solve the problem for us, thus relieving the federal government of responsibility to fund and administer a sustanined and rational fossil energy R&D program.

Unfortunately, I find it hard to believe that any of the three possibilities listed above contains any strong component of the truth. While it is true that gasoline and heating oil are temporarily somewhat less expensive than last year, it is also true that there has been no fundamental change in the finite nature of recoverable petroleum resources and that coal has not suddenly become easier, cheaper, or safer to mine, transport, burn, or transform in ways which are environmentally and economically acceptable. Thus, I must conclude that the long term fossil energy supply problem is just as serious as it was in 1982, and that was, in my estimation very serious indeed.

Furthermore, the quaint notion that the private sector will respond in a timely fashion with new technology to solve the problem on its own, is clearly untenable in the face of the number of oil shale and coal gasification and liquification programs abandoned by industry during the past year. This prospect is also dimmed by the snail's pace of major projects associated with the Synthetic Fuels Corporation. Furthermore, the generally poor economic climate has depressed an already inadequate level of industrially sponsored fossil energy R&D. The sole bright spot in the area of non-federal support is the on-going research agenda sponsored by industrial associates such as the Electric Power Research Institute and the Gas Research Institute. The programs sponsored by these industrial associations are important and welcome, but they are no substitute for a comprehensive federally supported program.

In short, the long term fossil energy problem has not been overstated, solved, or successfully transferred to someone else for solution. Thus, in my opinion, it is incumbent on the Department of Energy to mount a credible baseline research and technology development program for fossil energy supply and utilization problems. I seriously doubt that the proposed budget will allow DOE to achieve this goal and I would like to indicate at least a few specific areas where they will fail.

## Research Manpower and Equipment

While DOE has been guilty of wasting R&D funds in the past, principally by rushing prematurely into expensive large scale demonstration projects, it can be proud of stimulating the development of a skilled pool of scientists and engineers versed in fossil energy problems and skilled in the techniques required for their solution. This pool of technical talent was woefully shallow when the "energy crisis" emerged a decade ago. DOE funding for university, industry, and government laboratories has since stimulated the development of significant numbers of skilled people and the concomitant investment in laboratories and specifically designed for fossil energy research.

At the very least DOE should be sponsoring a fossil energy R&D program which insures the continued existence of those superior university, industrial, and governmental laboratories with demonstrated records of achievement in fossil energy research. The DOE program should also promote the continued training and equipping of superior scientists dedicated to solving key fossil energy programs.

The proposed FY '84 budget, coupled with last year's reductions, clearly informs this pool of technical talent and the organizations which recruited them and equipped their laboratories that the whole thing was a mistake. With this DOE budget proposal the current administration clearly signals its determination to dismantle the hard won competence achieved over the past ten years. This message is further amplified by continued reductions in allied programs sponsored by the Environmental Protection Agency and the National Bureau of Standards.

## Specific Program Weaknesses

In addition to generally threatening the national level of competence to deal with present and future fossil energy problems, the proposed budget eliminates or seriously reduces efforts in many specific areas of current concern. I find many of these reductions particularly hard to justify and would like to highlight a few:

1. In the area of Control Technology and Coal Preparation for direct coal utilization the proposed budget for Gas Stream Cleanup is cut 55% from FY '83 and the program on Cleanup Base Technology is eliminated entirely.

   The Gas Stream Cleanup program is aimed at removing noxious and/or corrosive species from the combustion streams of advanced coal utilization devices such as fuel cells or advanced gas turbine combined cycles. Such devices offer the best long term hope for both the efficient and economic conversion of coal to electricity and the solution of environmental problems, such as acid rain, caused by the emission of sulfur and nitrogen oxides, or health effects due to emission of heavy metal containing particulates. Many important basic questions involving homogeneous chemical and physical transformations, heterogeneous chemistry, and mass transport remain unanswered. With the budget more than halved, basic knowledge needed for the design and analysis of advanced systems will not be gained, thus further discouraging development efforts by utilities and their equipment vendors.

   The Cleanup Base Technology program was designed to help characterize, decontaminate, volume reduce, and dispose of the condensed phase coal preparation and coal combustion waste streams created by efforts to reduce sulfur oxide emissions from large coal combustion sources. This technology is urgently needed so that economic and environmentally sound methods of $SO_x$ reduction can be achieved. Need for these $SO_x$ reduction methods will be acute if current links of $SO_x$ emissions to serious health problems and/or serious acid rain effects are sustained. Elimination of all work on this portion of the $SO_x$ removal problem is extremely short sighted.

2. The Advanced Research and Technology Development program for coal is designed to provide the baseline research, for which even the current administration agrees it cannot expect the private sector to pay. Nevertheless this program is budgeted to suffer a 15% cut from FY '83 levels and a 45% cut from FY '82.

   Areas unadvisedly cut include the Materials program which is crucial and generic to the development of more efficient and environmentally acceptable advanced combustion, gasification and liquification systems. The Environmental Activities program is scheduled for halving in the face of increasing international concern over topics such as acid rain. And the Instrumentation, Control, and Diagnostics program is zeroed out despite the recognition that recent advances in instrumentally controlled combustion offer exciting advances in environmentally acceptable direct coal combustion.

3. The Magnetohydrodynamics (MHD) program, which offers the potential of very efficient coal to electricity conversion with negligible sulfur oxide emissions, clearly grew too big too fast. However, the proposed complete abandonment of a program this promising makes no sense. At the very least low level MHD technology base program should be funded to maintain, consolidate, and analyze the valuable data gained in the program to date.

I have highlighted unwise reductions in programs in which my colleagues and I have direct experience. I have no doubt that similarly unwise reduction for FY '84 are contained in other portions of the Fossil Energy Research and Development programs.

## Summary

In summary I believe the severe budget reductions proposed for the FY '84 Fossil Energy R&D program are unwarranted and unwise. If implemented they threaten specific program goals which are still as important as ever - if we wish to develop a safe, clean, economic, and assured supply of fossil energy. Furthermore, the proposed budget clearly threatens the continued existence of a stable, well trained, and well equipped pool of scientists and engineers capable of dealing with present and future fossil energy programs.

## DR. CHARLES E. KOLB

### EDUCATION

Ph.D. in Physical Chemistry, Princeton University, 1971
M.A. in Physical Chemistry, Princeton University, 1968
S.B. in Chemistry, Massachusetts Institute of Technology, 1967

### EXPERIENCE

Dr. Kolb is currently Vice President of Aerodyne Research, Inc. and Director of the Applied Sciences Division. He joined Aerodyne in 1971 as a Senior Research Scientist, and was appointed a Principal Research Scientist in 1975. From 1977 to 1979 he served as Director of the Division's Center for Chemical and Environmental Physics, while in 1979 and 1980 he served as Technical Director of the Applied Sciences Division.

Since joining Aerodyne, his personal areas of research have included atmospheric chemistry, combustion chemistry, chemical lasers, and the chemical physics of rocket and aircraft exhaust plumes. He is the author or coauthor of over thirty scientific publications in these fields.

Dr. Kolb has also served, since 1976, as an Associate in Atmospheric Chemistry in Harvard University's Center for Earth and Planetary Physics; and, since 1981, as a Research Affiliate in the Spectroscopy Laboratory at M.I.T. as well as a member of the Advisory Board for the M.I.T Regional Laser Center.

In the field of energy related combustion research, he initiated ARI's theoretical and experimental investigations into electron, negative ion, and slag condensation chemistry of coal combustion plasmas designed for magnetohydrodynamic power generation, and its programs in the evaluation and measurement of chemical kinetic and fluid dynamic parameters controlling pollutant formation, combustion induced corrosion, and fuel efficiency in coal and fossil fuel combustors.

Mr. **Fuqua**. Thank you very much, Dr. Kolb.

Dr. Leber, I understand you have two distinguished Floridians with you.

Mr. **Leber**. Yes. I will be happy to introduce them to you.

Mr. **Fuqua**. Please proceed.

## STATEMENT OF DR. ERIC LEBER, DIRECTOR OF ENERGY RESEARCH, AMERICAN PUBLIC POWER ASSOCIATION, ACCOMPANIED BY GEORGE RIZK, HEAD OF RESEARCH AND DEVELOPMENT, ENVIRONMENTAL DIVISION, JACKSONVILLE, FLA., ELECTRIC AUTHORITY; AND JOHN TWITCHELL, DIRECTOR OF ENGINEERING, SEMINOLE ELECTRIC COOPERATIVE

Dr. **Leber**. Mr. Chairman, I appreciate this opportunity to provide a few comments on the fiscal year 1984 fuel cell budget at the Department of Energy on behalf of the Fuel Cell Users Group of the electric utility industry.

Just a note about that Users Group: It does now comprise 55 privately and publicly owned utilities. Cumulatively, they provide about 40 percent of the electric power in this country.

If I could get on with the introductions, I am Eric Leber, and I am the director of energy research with the American Public Power Association. I do have two distinguished Floridians with me this morning. To my far right, we have Dr. George Rizk, who is the head of research and development in the environmental section of the Jacksonville, Fla., Electric Authority. Mr. Rizk will be our technical adviser this morning.

To my immediate right, I have John Twitchell, who is director of engineering with the Seminole Electric Cooperative based in Tampa. As you are surely aware, it supplies the electric power to about 47 of the rural counties in Florida.

Mr. **Fuqua**. And most of them in my district. [Laughter.]

Dr. **Leber**. So I would like to turn the microphone over to Mr. Twitchell, who will be providing the comments for the group this morning.

Mr. **Fuqua**. We are very pleased to have you here. I am very familiar with your organization and the new facilities that you are constructing at Palatka, plus over near Perry where your coal-fired plants will be coming on line.

Mr. **Twitchell**. Sir, I am pleased to report that our coal-fired plant in Palatka should generate its first kilowatt hour sometime this month.

Mr. **Fuqua**. Very good.

Mr. **Twitchell**. We should soon, hopefully, start seeing some relief from energy bills in rural Florida.

Mr. Chairman, I am sure you appreciate the outstanding potential of fuel cell technology, since you have cosponsored technical staff briefings on the subject for the past 2 years. The advantages of high efficiency, low environmental impact, short leadtime, fuel flexibility —which includes utilization of gasified coal or alcohols—modularity, and responsiveness make the fuel cell one of the more exciting commercial energy options for the 1990's and beyond.

There are also notable socio-economic arguments that can be made in favor of the fuel cell in view of the constructive role it could play at local, regional, and national levels. These points have been summarized in the letter attached to our testimony to Secretary Hodel of the Department of Energy from Mr. Lelan Sillin, chairman of the Users Group. They are also summarized in the resolution in support of "Research for Growth and National Security" passed by the membership of the National Rural Electric Cooperative Association at our recent 41st annual meeting, which is also attached to our testimony.

The promise of the fuel cell remains high. To achieve that promise by the end of this decade, sustained public and private partnership will be required. This means striving to achieve some level of consistency in the Federal funding profile, rather than the annual roller coaster effect that has characterized the program through recent years, and to maintain the strong, cooperative spirit that now exists between the public and private partners in this effort.

We estimate that the funding requirement for the Federal fuel cell program in fiscal year 1984 is $35 million. This is in stark contrast to the administration's $9.5 million request, but it is virtually identical to the average fuel cell budget for the past 6 fiscal years.

Moreover, since fuel cell funding has been practically constant since fiscal year 1978, our request clearly complies with the President's plea for a spending freeze in fiscal year 1984.

Of this $35 million total, we feel strongly that at least $22 million should be made available for the electric utility phosphoric fuel cell base programs currently under way and to bring to satisfactory completion the 4.8 megawatt project in New York City.

This is the level of support formally endorsed by the membership of the Users Group in approving, 2 days ago, the attached "Resolution on the Department of Energy Phosphoric Acid Fuel Cell Funding." At 63 percent of our total proposed budget, $22 million represents a slightly higher proportion of spending for phosphoric acid than the 58 percent in the fiscal year 1983 program. Yet it is significantly below the 81 percent dedicated to phosphoric acid by the Japanese in their aggressive fuel cell research, development, and demonstration program.

Our continuing strong interest in the development of the phosphoric acid fuel cell is based on the comparative readiness of this technology and the presence of committed U.S. companies in the area, which will help provide an efficient, competitive environment for the ultimate commercialization of the fuel cell.

As detailed in the findings of the "Planning Information Report for Development of Multi-Megawatt Fuel Cell Power Plants," which was prepared for the Department of Energy by the Users Group, the DOE program needs to focus on the multimegawatt phosphoric acid portion of the overall effort until success is in hand.

In the event that fuel cell funding can be made available in excess of the $35 million required for fiscal year 1984, we would strongly urge the committee to provide an additional $6 million in advanced research and technology development funds to support basic research in phosphoric acid and advanced concept fuel cell powerplant materials and processes.

No less than $4 million would be provided to the private sector for technology advancement of phosphoric acid fuel cells. Specific areas of investigation would be: catalyst development with higher performance and lower utilization of strategic materials; development of materials, processes, and replenishment concepts to provide extended acid life; development of advanced fuel cell materials and fabrication processes for electrodes, separators, and cooling components; development of more efficient and durable fuel cell electrode structures; development of improved fuel processing techniques; and, finally, development of advanced control technology.

Private sector research and development contributions for fiscal year 1984 are expected to remain higher than Federal outlays, approaching the roughly 2-to-1 ratio maintained throughout the history of the fuel cell program. The negotiations presently under way regarding four or five prototype fuel cell demonstrations represent an additional private sector expenditure of more than $300 million.

Utilities, individually and through the Electric Power Research Institute, as well as the prospective manufacturers, are keeping steadfast in their commitment to the fuel cell in general and in their resolve for early commercialization of the multimegawatt phosphoric acid fuel cell in particular.

We appreciate this opportunity to have made some comments before the committee.

Mr. FUQUA. Does that conclude your statement, Dr. Leber?

Dr. LEBER. It does.

[The prepared statement of the Fuel Cell Users Group follows:]

# STATEMENT ON THE FY 1984 FUEL CELL BUDGET OF THE DEPARTMENT OF ENERGY

Presented to the
SUBCOMMITTEE ON ENERGY DEVELOPMENT AND APPLICATIONS
of the
COMMITTEE ON SCIENCE AND TECHNOLOGY
U.S. House of Representatives

by the
FUEL CELL USERS GROUP OF THE ELECTRIC UTILITY INDUSTRY, INC.
March 17, 1983

Mr. Chairman, thank you for this opportunity for the Fuel Cell Users Group of the Electric Utility Industry, Inc. (the Users Group)[1] to present these brief remarks on the FY 1984 Department of Energy fuel cell budget. I am Eric Leber, Director of Energy Research with the American Public Power Association; accompanying me this morning are Mr. George Rizk, Head of the Research and Development Section in the Environmental Division of the Jacksonville, Florida, Electric Authority and Mr. John Twitchell, Director of Engineering at the Seminole Electric Cooperative based in Tampa, Florida. Mr. Twitchell will be presenting the Users Group's comments.

Mr. Chairman, I am sure you appreciate the outstanding potential of the fuel cell technology, since you have cosponsored technical staff briefings on the subject for the past two years. The advantages of high efficiency, low environmental impact, short lead time, fuel flexibility, cogeneration, modularity, responsiveness, and others combine to make the fuel cell one of the more exciting commercial energy options for the 1990s and beyond.[2] There are also notable socio-economic arguments that can be made in favor of the fuel cell, in view of the constructive role it could play at local, regional, and national levels. These points have been summarized in the attached letter (Attachment 2) to Secretary Hodel of the DOE from Mr. Lelan Sillin, Jr., Chairman of the Users Group. They are also summarized in the Resolution in support of "Research for Growth and National Security" passed by the Membership of the National Rural Electric Cooperative Association (NRECA) at its 41st Annual Meeting, January 30-February 3, 1983 and also attached (Attachment 3).

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[1] The Fuel Cell Users Group of the Electric Utility Industry, Inc. is a three-year-old organization with a membership of 55 utilities (and eight Associate Members) spanning the privately, publicly, and cooperatively owned electric utility industry. A list of our membership is included as Attachment 1. Cumulatively, the members of the Users Group supply about 40% of the electric energy in this country. We are united in the interest of encouraging and supporting research, development, and demonstration leading to the ultimate commercialization of the phosphoric acid fuel cell power plant.

The promise of the fuel cell remains high. To achieve that promise by the end of this decade, sustained public/private partnership will be required. This means striving to achieve some level of consistency in the Federal funding profile--rather than the annual "roller coaster" effect that has characterized the program through recent years--and to maintain the strong, cooperative spirit that now exists between the public and private partners in this effort.

We estimate that the funding requirement for the federal fuel cell program in FY 1984 is $35 million. While this is in stark contrast to the Administration's $9.5 million request, it is virtually identical to the average fuel cell budget for the last six fiscal years. Moreover, since fuel cell funding has been practically constant since FY78, our request clearly complies with the President's plea for a spending freeze in FY84.

Of this $35 million total, at least $22 million should be made available for the electric utility phosphoric acid fuel cell base programs currently underway and to bring to satisfactory completion the 4.8 MW project in New York City. This is the level of support formally endorsed by the Membership of the Users Group in approving two days ago the attached "Resolution on DOE Phosphoric Acid Fuel Cell Funding" (Attachment 4). At 63% of the total budget, $22 million represents a slightly higher proportion of spending for phosphoric acid than the 58% in the FY83 program. Yet it is significantly below the 81% dedi-

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[2] In his statement before this Committee last week, Mr. Jan Mares, Assistant Secretary of the DOE for Fossil Energy, referred to the fuel cell as "a revolutionary hydrocarbon fuel conversion technology to increase the cost-effective, efficient, and environmentally acceptable use of conventional and alternative hydrocarbon fuels, thereby reducing demand for oil."

cated to phosphoric acid by the Japanese in their aggressive fuel cell research, development, and demonstration program.

Our continuing strong interest in the development of the phosphoric acid fuel cell is based on the comparative readiness of this technology and the presence of committed U.S. companies in the area, which will help provide an efficient, competitive environment for the ultimate commercialization of the fuel cell. As detailed in the findings of the "Planning Information Report for Development of Multi-Megawatt Fuel Cell Power Plants" (March, 1983) prepared for the Department of Energy by the Users Group, the DOE program needs to focus on the multi-megawatt phosphoric acid portion of the overall effort until success is in hand.

In the event that fuel cell funding can be made available in excess of the $35 million required for FY84, we strongly urge the Committee to provide $6 million in advanced research and technology development funds to support basic research in phosphoric acid and advanced concept fuel cell power plant materials and processes. No less than $4 million would be provided to the private sector for technology advancement of phosphoric acid fuel cells. Specific areas of investigation would be: (1) catalyst development with higher performance and/or lower utilization of strategic materials; (2) development of materials, processes, and/or replenishment concepts to provide extended acid life; (3) development of advanced fuel cell materials and fabrication processes for electrodes, separators, and cooling components; (4) development of more efficient and durable fuel cell electrode structures; (5) development of improved fuel processing techniques; and (6) development of advanced control technology.

Private sector R&D contributions for FY84, meanwhile, are expected to remain higher than federal outlays, approaching the roughly 2:1 ratio maintained throughout the history of the fuel cell program. (Through 1982, DOE funding totaled $95.1 million with the private sector share at $184.1 million.) Furthermore, the negotiations presently underway regarding four or five prototype fuel cell demonstrations represent an additional private sector expenditure of more than $300 million. The utilities--individually and through the Electric Power Research Institute--as well as the prospective manufacturers are keeping steadfast in their commitment to the fuel cell, in general, and in their resolve for early commercialization of the multi-megawatt phosphoric acid fuel cell, in particular.

Mr. Chairman, I thank you and the Committee for your longstanding support of this attractive technology. I encourage your continued funding for the next few years. And I appreciate the time to present these remarks.

Attachment 1

## FUEL CELL USERS GROUP MEMBERS

Adams Electric Cooperative

Allegheny Electric Cooperative

American Public Power Association

Anchorage Municipal Light & Power

Arizona Public Service Company

Baltimore Gas & Electric Company

Basin Electric Cooperative

Boston Edison Company

Brazos Electric Power Cooperative

Buckeye Power, Inc.

Central Hudson Gas & Electric Corp.

Central Illinois Light Company

Colorado Ute Electric Assn., Inc.

Consolidated Edison Co. of NY

The Dayton Power & Light Company

Delmarva Power & Light Company

Easton Utilities Commission

Edison Electric Institute

Electric Power Research Institute

Green Mountain Power Corporation

Hawaiian Electric Company

Hoosier Energy Division, REC, Inc.

Hydro-Quebec

Idaho Power Company

Jacksonville Electric Authority

Kansas City Power & Light Co.

Lee County Electric Cooperative

Lincoln Electric System

Long Island Lighting Company

Massachusetts Municipal Wholesale Electric Company

Memphis Light, Gas, and Water Division

Mississippi Power & Light Company

Missouri Basin Municipal Power Agency

National Rural Electric Cooperative Association

Niagara Mohawk Power Corporation

Northeast Utilities Service Co.

Ohio Edison Company

Ontario Hydro

Philadelphia Electric Company

Provo City Power

Public Service Company of Oklahoma

Public Service Electric & Gas Co.

Rural Electrification Administration

San Diego Gas & Electric Company

Santa Clara Electric Department

Seminole Electric Cooperative

Southern California Edison Company

Southern Company Services, Inc.

Southern Maryland Electric Coop.

Tampa Electric Company

Taunton Municipal Lighting Plant

Tennessee Valley Authority

Toledo Edison Company

United Power Association

Virginia Electric Power Company

Associate Members

Burns & McDonnell Engineering Company

Burns & Roe, Inc.

Ebasco Business Consulting Services, Inc.

Johnson Matthey Research Centre

Kinetics

New Energy Development Organization

NUS Corporation

Stone & Webster Engineering Corporation

Attachment 2

**THE FUEL CELL USERS GROUP**
OF THE ELECTRIC UTILITY INDUSTRY, Inc

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BOARD OF DIRECTORS

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January 31, 1983

The Honorable Donald P. Hodel
Secretary
U.S. Department of Energy
1000 Independence Avenue, S.W.
Washington, DC 20585

Dear ~~Secretary~~ Don Hodel:

I am writing to introduce the Fuel Cell Users Group of the Electric Utility Industry, Inc. and to discuss the continuing need for a strong partnership between the Federal government and the utility industry for the advancement of the fuel-cell technology.

The Users Group is a three-year-old organization with a membership of 55 utilities (and 8 Associate Members) spanning the privately, publicly, and cooperatively owned electric utility industry. Cumulatively, the members of the Users Group supply about 40% of the electric energy in this country. We are united in the interest of encouraging and supporting research, development, and demonstration leading to the ultimate commercialization of the phosphoric acid fuel-cell power plant.

You're probably aware of many of the features of the fuel cell that make it attractive to utilities. Among these are:

1. High efficiency (projected to be greater than 40% for electricity production)
2. Environmental compatibility
3. Short lead time
4. Fuel flexibility
5. Cogeneration potential (resulting in combined heat and power efficiencies exceeding 80%)

55%

6. Siting flexibility
7. Modular construction
8. Reduced capital requirements for plant and transmission/distribution equipment
9. Fast transient response
10. Load-following capability
11. Decreased requirements for spinning capacity reserves
12. Reduced line losses
13. Automatic operation
14. Improved power system stability

These items are discussed in more detail in the enclosed guide, "Fuel Cell Power Plants for Dispersed Generation," from the Electric Power Research Institute.

Beyond these mechanical and operational characteristics there are a number of important social and economic questions that deserve consideration. A discussion of the fuel cell in the context of these broader issues is appended for your consideration.

These many compelling advantages--technical and socioeconomic--of the fuel cell over other generating systems factored heavily in the Congress' continued support of federal funding for all fuel cell activities in the range of $30-35 million per year for the past several years. However, DOE budget requests have not been equally stable, and so the fuel cell budget is subject to the "roller-coaster" effect you noted during your confirmation hearings. Needless to say, I'm encouraged by your resolve to control these funding transients and provide stability and consistency in the Department's programs.

These same attractive fuel-cell attributes also inspired the Energy and Water Subcommittee of House Appropriations to target fuel-cell based cogeneration as one of two technical initiatives (the other being heat pumps) particularly worthy of consideration by the Bonneville Power Administration under the mandates of the Regional Power Planning and Conservation Act.

The private sector (here consisting of all segments of the utility industry, the Electric Power Research Institute, and prospective fuel-cell manufacturers) is seriously committed to advancing the fuel cell to commercial status. As evidence of the level of our commitment, over the past ten years the private sector partners have spent about two dollars for every dollar of federal expenditure, a ratio that is expected to be maintained in the future.

Moreover, as vital research and development continue on the fuel-cell concept and components, progress is being made on the demonstration of four or five early prototype fuel cells. These efforts (at privately, municipally, and cooperatively owned utilities--Southern California Edison and Niagara Mohawk Power Corporation, New York; Santa Clara, California, Electric Department; and the United Power Association in Minnesota; respectively) are being pursued above and beyond the testing of 4.8 MW fuel-cell systems at Consolidated Edison Company in New York and the Tokyo Electric Power Company. It is anticipated that these new initiatives may represent more than $300 million in utility outlays.

It is through a strong, sustained partnership that these various current and anticipated endeavors will bring to fruition this promising technology. Programmatic, as well as fiscal support is required. As we proceed in our R,D&D activities and look toward fiscal year 1984, I hope the industry and the Users Group, in particular, can count on the cooperation of the fuel-cell and advanced research and technology development programs of the Fossil Energy Office and a consistent level of funding near those of FY-82 or FY-83 (in the range of $30-$35 million). Our request, by the way, is fully in accord with the President's recent appeal for a spending freeze in FY-84; it would indeed hold the line on--if not slightly decrease--real outlays for the fuel-cell program.

I'd like to note, in reflecting on the recent State of the Union message, that the fuel cell is a prime example of the type of high technology to which the President was referring. If we can maintain our partnership between public and private sectors in this work, we'll help keep America the technological leader of the world, as the President so fervently desires.

If I can provide any further information or respond to any questions regarding the fuel cell or the Fuel Cell Users Group, please don't hesitate to contact me.

Sincerely,

Lee

Lelan F. Sillin, Jr.
Chairman

LFS/lj
Enclosures

# A BRIEF REVIEW OF FUEL-CELL CHARACTERISTICS FROM THE PERSPECTIVE OF NATIONAL SECURITY, SAFETY AND THE GENERAL WELFARE

Submitted by
The Fuel Cell Users Group of the Electric Utility Industry, Inc.

January 31, 1983

I. National Security: dispersed, stand-alone, fuel-cell energy systems can help diminish national vulnerability to prolonged, disruptive power outages due to accidents, terrorist attacks on electric transmission systems, or EMP (Electromagnetic Pulse) effects from nuclear detonations.

By virture of its inherent fuel flexibility, the fuel cell can be the foundation for independent islands of energy self-sufficiency unconstrained by an aggravated reliance on long-distance transportation or a single fuel source (be it oil, gas, or coal).

In the Energy Security Act, the Congress and Administration underscored energy diversity as key to energy security. The fuel cell could be an important component in achieving this energy diversity.

II. Economic wellbeing: consistent with the President's Enterprise Zones plan, the fuel cell could serve as the nucleus for commercial activity, jobs, and redevelopment in urban, as well as non-urban, areas.

Economic revitalization could be more readily achieved through the deployment of cogenerating fuel-cell energy systems (with both electric and thermal outputs) in the inner-city neighborhoods where there might be insufficient space, services, and resources to support a conventional power plant.

This coupling of local energy availability to economic vitality is enjoying a growing level of support within many state and local jurisdictions, though it currently remains absent from the Administration's plan.

III. Adequacy of domestic supplies of fuel: Oil, gas, and even coal are of finite supply. Other fuel sources are renewable: gases from biological processes (such as sewage treatment plants or landfills), alcohols from biomaterials, methane from coal seams and shales, hydrogen from biological or electrochemcial sources, etc. All are suitable for the fuel cell.

The question of fuel availability must go beyond considerations of the proven size of the domestic resource to include considerations of the stability and reliability of the supply infrastructure for that fuel. It is not enough to know that deposits of a particular fuel abound within the continental United States if the means to bring that fuel to the consumers are inadequate or undependable.

Research and development on the fuel cell is focusing on producing an adaptable, resilient energy system that will handle a range of locally available fuels (carbon- or hydrogen-based gases, liquids, or solids). The fuel cell will have the capacity to switch from one fuel to another in periods of a few hours with modest associated capital costs (roughly a 5% increment).

IV. Environment, health, and safety: The fuel cell is clearly superior to existing fossil-fired options. Emissions should be significantly below any regulatory requirements; further, the fuel cell is quiet and uses little water.

Attachment 3

NRECA Annual Meeting 1983

RESOLUTION J-2

FROM: The Research & Technological Development Committee
APPROVED BY: Resolutions Committee

SUBJECT: Research for Growth and National Security

President Reagan, in his recent State of the Union Address, emphasized the need for greater attention to research as "the tool for growth." We commend the President for voicing strong support for research; and also we commend the Democratic party, which in its response statement, listed "Research and Development" as the number one priority for enhancing the long-term economic welfare of this nation.

We subscribe to this philosophy and urge prompt by-partisan action by the Administration and the Congress to establish a strong, well-funded federal research and development program.

Specifically, we support a well-funded, long-range commitment for Electric Energy Systems (EES) research and development and fuel cell development. These areas have national security implications as well as the potential for providing opportunities for growth and energy self sufficiency.

We therefore urge the following:

*Minimum funding of $32-million for FY-84 for Electric Energy Systems research within the Department of Energy. EES is involved with several programs of critical national importance. Among them is the study of the effects of Electro Magnetic Pulse (EMP) which in the event of strategic military weapon explosions in the atmosphere, could result in a pulse from scattering gamma rays that could create a surge 10 to 100 times faster than a lightning stroke. Such a pulse could disrupt communications and immobilize the entire electric utility industry. Other EES projects which require strong support include system technologies involving improving efficiencies and reliability of power generation, transmission and energy storage, and the electric field effects of high voltage transmission lines.

*A five year commitment, with a minimum of $33-million for FY-84, for fuel cell research and development. The potential offered by fuel cell technology for highly efficient, dispersed electric power generation (so essential to national security) is enormous. Along with the potential economic advantages, the fuel cell has other very desirable characteristics (environmentally benign, short lead time for installation, modular design, and favorable load-following capabilities) which makes it truly a "tool for growth."

Attachment 4

## RESOLUTION
## ON DOE PHOSPHORIC ACID FUEL CELL FUNDING

The Fuel Cell Users Group of the Electric Utility Industry, Inc.:

1. Having brought together all segments of the industry to promote commercial fuel cell applications that best meet the industry's requirements;
2. Believing that the efficiency, fuel flexibility, modularity, and environmental characteristics inherent in the fuel cell offer significant benefits to the industry, and more importantly, to its customers;
3. Recognizing that a successful development program is a prerequisite to commercialization, and that the Department of Energy and the industry share in the funding of such a program;
4. Being concerned that the proposed Department of Energy budget for fiscal year 1984 falls short of the funding necessary to sustain the development program at an optimum level; therefore

RESOLVES, that the Fuel Cell Users Group of the Electric Utility Industry, Inc.:

1. Believes that the fiscal year 1984 Department of Energy Fuel Cell Budget should include $22 million for development of the electric utility phosphoric acid fuel cell, and
2. Strongly urges the Congress to make these funds available.

Adopted
March 15, 1983

Robert A. Bell
President
Fuel Cell Users Group of the
Electric Utility Industry, Inc.

Mr. FUQUA. Now we will hear from Mr. Jones.

## STATEMENT OF MALCOLM S. JONES, JR., MANAGER OF ADVANCED TECHNOLOGY AND SPECIAL PROJECTS, EBASCO SERVICES INC.

Mr. JONES. I am Malcolm Jones of Ebasco Services, Inc., in Santa Ana, Calif. Most of our family live in Florida, and we are kind of displaced from the home base, being out there.

I am appearing here today in behalf of a group we call the Industrial Fuel Cell Users Group. It is a new organization which I will talk about. It is a voluntary association of individuals in several industries which was established to foster utilization of fuel cells in American indsutry.

Members represent the petroleum industry, the chemical process industries, the metallurgical industries, and other potential users of fuel cells in the nonutility context. I have a list of the present and pending members which is in the written testimony.

Dr. Gordon Liu of Dow Chemical Co. in Midland, Mich., is serving as the interim chairman of the organization until we get fully organized and have an infrastructure in place, like the Electric Utility Fuel Cell Users Group, to handle the technical and managerial aspects of the organization. We held our first meeting last fall at the fuel cell seminar in Newport Beach, Calif., and the membership has been growing rapidly.

The group's objectives are twofold. The first is to make industry aware of the advantages which may be realized through the commercial application of fuel cells in the respective industrial segments. It is believed that through the use of fuel cells these industries may be more energy and capital efficient, and hence more competitive in today's markets. This will assist these industries in both the U.S. markets, which can mean improved standards of living and jobs for those of us in the United States, and in world markets with a corresponding benefit to the U.S. balance of trade.

It is our hope that this can be done with fuel cell systems which are manufactured in the United States. Fuel cell systems are one example of the high technology industries where the United States could excel in the coming two decades, if we have the national will to do so.

The second goal is to serve as a forum for defining to the equipment manufacturers what type of fuel cell equipment and systems that industrial users would like to see in the marketplace.

These goals are similar to those of the two other fuel cell users groups: the Electric Utility Fuel Cell Users Group, which is supported by EPRI, and the On-Site Fuel Cell Users Group, which is supported by the Gas Research Institute.

It is of interest to observe how and why these users groups came about. The early fuel cell developments were directed at the space program and for military applications. As the technology grew, equipment manufacturers identified the electric utility and gas utility industries as the largest potential markets for fuel cells.

In cooperation with respective industry groups and the Federal Government, development was started on fuel cell systems for these two applications. The Consolidated Edison 4.8 megawatt demonstration and the 40 kilowatt on-site demonstration plants are products of this cooperation.

There has been widespread support for these developments within both the electric utilities and the gas utilities. Initially, there were a few utilities who strongly supported fuel cell development. As interest spread, it became clear that there was a need for a mechanism which would allow a wider level of participation by many utilities. As we have just heard, there are now 55 that are participating.

Much of the coordination of the industry effort is now accomplished through the Electric Utility Fuel Cell Users Group and the On-Site Fuel Cell Users Group. Both have played an active role in defining utility industry requirements for the equipment manufacturers and quantifying the benefits which the individual users might hope to obtain through the use of fuel cells. Such user-manufacturer interactions are important to insure that whatever equipment which is finally introduced into the marketplace will meet user needs.

There are two important differences between these two user groups and our Industrial Fuel Cell Users Group. The first is that these groups represent organizations which do, or desire to, produce electricity to consumers, whereas our group represents industries that are major consumers of electrical energy and who may, under certain conditions, find it in their best economic interests to generate power for some of their own electricity needs.

The second difference is that there is no single focus industrial research organization such as EPRI or GRI to support the energy R&D requirement of these industries. There are some industrywide organizations such as the Chlorine Institute, the API, the Aluminum Association, the Manufacturing Chemists Association, API, which deal primarily with issues such as industry product standards, process equipment standards, market statistics, et cetera, but not the energy R&D for these industries.

It was felt that an Industrial Fuel Cells Users Group, as differentiated from the Utility Users Groups, could perform a similar function and serve as the focal point for establishing the specifications and requirements for fuel cell systems which would be sold to industrial organizations who wish to generate some of their own power.

In the mature power generation technologies, organizations such as the American Society of Mechanical Engineers, International Standards Organization, have established performance standards for items of equipment for such turbines. But with fuel cells, we are dealing with a complete system where the fuel cell is only one component, and such standards have not been established.

At this point, I think it is perhaps appropriate that I bring to the committee's attention some of the benefits which American industry could realize through the use of fuel cells. Looking at some of the potential applications will make it clear why the specifications for fuel cells and fuel cell systems to be used by industry may necessarily be different from those used by the utilities.

A primary example is the electrochemical industry, and particularly the chlor-alkali industry, where salt brine is converted into chlorine and caustic soda, which is sodium hydroxide, by electrolysis. One of the advantages of fuel cells for this application is that they generate direct current.

One of the things which is not readily appreciated is the large fraction of the electrical power produced in the United States that is consumed as direct current. Between the aluminum industry, the chlor-alkali industry, the plating industry, and other metallurgical applications, about 8 percent of all electricity in the United States is used as direct current. This means that about 1 in 12 kilowatthours end up as direct current at the point of consumption. If you have a generating device which produces direct current, such as fuel cells, MHD, thermionics, then these would be the preferred technologies for providing power for these applications. For the current and voltage levels at which the electricity is used, it is not practical to transmit the direct current any appreciable distance, so it would have to be generated on site with plants normally running in the range of 20 to 100 megawatts.

I may get slightly technical for a minute. I have a flow chart which is contained in the presentation which shows the use of a fuel cell for providing power for a 1,000 tons per day chlor-alkali plant. It is a conventional technology with other technologies; it might vary slightly.

As shown, a major part of the electricity, about 80 percent of it, is consumed as direct current, with 20 percent being converted to alternating current to power various pumps and motors.

One energy input to the chlor-alkali plant is steam, which is used to treat the brine and to concentrate the caustic soda product for shipment. This steam can be supplied from waste heat generated in the fuel cell, a cogeneration aspect which further improves efficiency.

One important byproduct of the electrolysis of brine is the hydrogen which is produced. In normal chlor-alkali plants it is burned under a boiler to produce part of the steam required for product concentration, or simply flared. With the fuel cell plants, this hydrogen can be cleaned up, pressurized, and used as fuel in the fuel cell.

This may look like perpetual motion, but it really is not. Only about 16 percent of the energy can come from the hydrogen, but it is still an important fuel saving.

At this point, we may note the important differences between the 100-megawatt fuel cell system required for this hypothetical chlor-alkali plant and the fuel cell systems which would be specified for the electric utility applications. These are: Most of the electricity is used as direct current and only a small amount converted to alternating current, which the electric utilities do; there is a need for cogenerated steam, which is not provided by the electric utility fuel cell plant; and not all of the input passes through the fuel processor.

Similar analyses could be performed for a refinery where they have streams of waste hydrogen which could be converted, or if the molten carbonate fuel cells, which are one of the advanced technologies, were available, for aluminum production from bauxite, the cogeneration would make those kinds of applications very attractive along our gulf coast.

The major point I would like to make is that these industrial applications, for the most part, involve cogeneration. Cogeneration, can provide attractive rates of return to those industries which could and use fuel cells if they were commercially available now. As with any of the emerging energy conversion technologies, again the MHD and thermionics, the industrial cogeneration applications look like the most likely to succeed areas because of the leverage which they have over the generation of electricity and thermal power in separate installations.

While the electric utility market for fuel cells may ultimately be larger, I feel that the market for fuel cells to be used in industry is much nearer.

This is one reason I am pleased to be able to appear before this committee and comment on the administration's proposal for fuel cell research and development activities in fiscal year 1984.

With respect to the program which has been proposed, I do not concur with the concept that the present activities be drastically curtailed at this point. We are at the point where the first generation technology is ready to be demonstrated in the field. While it may appear to be an expensive communication method, there is nothing like working hardware to get the message across to the decisionmakers in private industry. It is these decisionmakers, both manufacturer and user, who will ultimately decide the fate of the fuel cell industry in the United States.

I would respectfully recommend that the DOE program include some small level of effort to be directed at further definition of the industrial and cogeneration fuel cell application areas in order to more clearly define the equipment needs of the respective industry segments.

I have found that investigative studies which are cofunded by both the Government and industry develop a high level of awareness within the industry of both the advantages and the disadvantages represented by a given technology.

The real problems emerge and the potential benefits get quantified in a much different way than has happened in previous Government efforts, such as the ECAS studies, where the potential equipment suppliers were conducting the surveys.

Having these studies conducted by or through the respective industry associations would bring a broad level of opinion to bear on the topic and would provide a ground for industry consensus on the applicability of the technology, and in what form. This would provide the equipment manufacturers with a clear guide for assessing the nonutility market and for fashioning products and systems for that market.

While our Industrial Fuel Cell Users Group will eventually develop the same criteria over the period of several years, I am concerned about the timeliness of the information in order to have an impact on U.S. manufacturers before foreign manufacturers have moved strongly into the field. For this reason, I recommend that these activities be identified as a line item in the Department of Energy budget and be afforded a modest level of support.

I believe that as the program develops and the benefits become clearer, there will be much more incentive for industrial concerns or groups to participate in, and fund, a larger share of the costs of demonstration of fuel cells systems and concepts. This will be because the benefits to industry will be achievable at a lower level of technology development than would be required for the non-industry applications.

An important part of any demonstration program is that there is hardware to demonstrate. To this end, I support the continued development of advanced concepts such as the improved phosphoric acid fuel cell, the molten carbonate fuel cell, and the solid electrolyte fuel cell.

From an industrial users standpoint, development of the improved phosphoric acid cell is the most important to the utility applications, and they, through their research organizations, have a strong interest in seeing it developed. From an industrial users standpoint, many of our members would be just as happy if the present generation of phosphoric acid fuel cell stack assemblies were available at commercial prices.

Over the long run, the molten carbonate and solid oxide fuel cells will probably be most important to industrial users because of their higher efficiency and the fact that the waste heat is available at higher temperatures, which makes it more valuable for process heat applications in many industries. These considerations are not as important for the utility applications.

We, therefore, strongly recommend that technology development on these systems be continued until they are ready for the industrial marketplace.

One of the planned activities of our Industrial Fuel Cell Users Group is to further define the benefits to industry which could accrue through industrial adaptation of these technologies. Because of the high risk nature of these two technologies, the solid oxide and the molten carbonate, I do not see industrial users pursuing technology development in these areas, except where they are component suppliers, but only participating in demonstration activities after the hardware is developed.

Thus far, the primary emphasis on fuel cell system development has been as a replacement of central station electrical power generation. For these applications, the source of electricity is transparent to the user, who has no way of knowing whether it is from a nuclear plant, coal-fired plant or dam or from a fuel cell. The only way he knows is when he gets his bill.

The point I am trying to make is that for these applications, the fuel cell system is trying to compete with conventional power sources which have been fully developed to mature technologies over a 100-year period, with efficiencies honed to a fraction of a decimal point, capital costs minimized with multicomponent variational analysis, and operating experience which projects 30- or 40-year economic life.

For these reasons, it is believed that the use of fuel cells in the industrial cogeneration mode is more likely to succeed in a commercial sense at an early level of development.

While some of the administration may not agree, there is the view that it may be part of the Federal Government's role to provide support for front-end development efforts which will allow fuel cell systems an opportunity to compete with these mature systems in this market.

To justify Federal support for the development of fuel cells, an interesting precedent may be drawn with respect to gas turbines for the electric utility applications. Similar to fuel cells, gas turbines were first developed for governmental applications: the gas turbine to power jet fighters and the fuel cells for power in space.

The amounts of Federal funds spent on developing jet engines for military programs were nontrivial, as you Members well know. After gas turbines had reached a fairly high level of technical development for the military, they were then presented in the commercial market to the electric utilities as aircraft derivative engines, with essentially 99 percent of their development covered by military appropriations.

There has been some military development of fuel cell systems for both propulsion and for quiet, portable power sources, but this level of activity is still about four orders of magnitude less than what is presently going into military gas turbine—jet engine—development.

One important consideration may be that the technology base being developed by the Department of Energy under this program is directly applicable to Department of Defense missions. This may be cited as another reason for continuing Federal support of fuel cell system development at its present level of activity.

I see a parallel between the development of the gas turbine and the phosphoric acid based fuel cell systems. In many ways, the phosphoric acid fuel cell systems are similar to gas turbines in their potential roles within the electric utility industry. They both essentially offer low capital cost installations but require a premium fuel, with the fuel cell system offering slightly higher efficiency than the gas turbine-combined cycle plant.

I feel that the fuel cell systems will eventually play a similar role to gas turbines in the utilities' overall generation resource plan and, as such, will not be used as base load units. However, for industrial applications, high load factors, or what could be called base loading is a general rule. Therefore, the design of the fuel cells themselves for these applications may be different than the designs for utility applications, which is not the case with gas turbines.

I appreciate the opportunity to address the committee and express my views on fuel cells for industrial applications. For those who are interested in more technical details, we have some papers which we could pass out.

Are there any questions?

[The prepared statement of Mr. Jones follows:]

# TESTIMONY

OF

MALCOLM S. JONES, JR.
EBASCO SERVICES INCORPORATED

SPEAKING FOR THE INDUSTRIAL FUEL CELL USERS GROUP

TO

THE HOUSE COMMITTEE ON SCIENCE AND TECHNOLOGY
SUBCOMMITTEE ON ENERGY DEVELOPMENT AND APPLICATIONS

MARCH 17, 1983

# STATEMENT OF MALCOLM S. JONES, JR.

Mr. Chairman, and Members of the Subcommittee:

I am Malcolm Jones of Ebasco Services Incorporated, from Santa Ana, California. I am appearing here today in behalf of a group which we call the Industrial Fuel Cell Users Group. This group, which is a voluntary association of individuals in several industries, was established to foster utilization of fuel cells in American Industry. Members represent the petroleum industry, the chemical process industries, the metallurgical industries, and other potential users of fuel cells in the non-utility context. A written list of present and pending members is presented in the written testimony. Dr. Gordon Liu of the Dow Chemical Company in Midland, Michigan, is serving as interim chairman of the organization until we get fully organized and have an infrastructure in place to handle the technical and managerial aspects of the organization. We held our first meeting last fall at the Fuel Cell Seminar in Newport Beach, California, and the membership has been growing rapidly.

The group's objectives are twofold. The first is to make industry aware of the advantages which may be realized through the commercial application of fuel cells in the respective industrial segments. It is believed that through the use of fuels cells, these industries may be more energy and capital efficient, and hence more competitive in today's markets. This will assist these industries in both the U. S. markets, which can mean an improved standard of living and jobs for those of us in the United States, and in world markets, with a corresponding benefit to the U. S. balance of trade. It is our hope that this can be done with fuel cell systems which are manufactured in the United States. Fuel cell systems are one example of the high technology industries where the U. S. could excell in the coming two decades, if we have the national will to do so.

The second goal is to serve as a forum for defining to the equipment manufacturers what type of fuel cell equipment and systems that industrial users would like to see in the marketplace.

These goals are similar to those of the two other fuel cell users groups: the Electric Utility Fuel Cell Users Group, which is supported by the Electrical Power Research Institute, and the On-Site Fuel Cell Users Group, which is supported by the Gas Research Institute.

It is of interest to observe how and why these Users Groups came about. Early fuel cell developments were directed at the space program and for military applications. As the technology grew, equipment manufacturers identified the electric utility and the gas utility industries as the largest potential markets for fuel cells. In cooperation with respective industry groups and the Federal Government, development was started on fuel cell systems for these two applications. The Consolidated Edison 4.8 MWe demonstration plant, and the 40 kWe On-Site Demonstration plants are present products of this cooperation.

There has been widespread support for these developments within both the electrical utilities and the gas utilities. Initially there were a few utilities who strongly supported fuel cell development. As interest spread, it became clear that there was a need for a mechanism which would allow a wider level of participation by many utilities, and therefore the users groups were started. Much of the coordination of the industry effort is now accomplished through the Electric Utility Fuel Cell Users Group and the On-Site Fuel Cell Users Group. Both have played an active role in defining utility industry requirements for the equipment manufacturers, and in quantifying the benefits which the individual users might hope to obtain through the use of fuel cells. Such user-manufacturer interactions are important to insure that whatever equipment which is finally introduced into the marketplace will meet the user's needs.

There are two important differences between these two users groups and our Industrial Fuel Cell Users Group. The first is that these groups represent organizations which do, or desire to produce electricity for sale to consumers, whereas our group represents industries which are major consumers of electrical energy, and who may, under certain conditions, find it in their best economic interests to generate power for some of their own electricity needs. The second difference is that there are no single focus industrial research organizations such as EPRI or GRI to support the energy R & D requirements for these industries. There are some industry wide organizations such as the Chlorine Institute, the American Petroleum Institute, the Aluminum Association, the Manufacturing Chemists Association, the American Paper Institute, etc., which deal primarily with issues such as industry product standards, process equipment standards, market statistics, etc., not energy R & D for these industries. It was felt that an Industrial Fuel Cell Users Group, as differentiated from the Utility Users Groups, could perform a similar function and serve as a focal point for establishing the specifications and requirements for fuel cell systems which would be sold to industrial organizations who wish to generate some of their own power. In the mature power generation technologies, organizations such as the ASME, ISO, etc., have established performance standards for items of equipment such as turbines, etc. But with fuel cells we are dealing with a complete system, where the fuel cell is only one component, and such standards have not been established.

At this point it is perhaps appropriate that I bring to the Committee's attention some of the benefits which American industry could realize through the use of fuel cells. Looking at some of the potential applications will make it clear why the specifications for fuel cells and fuel cell systems to be used in industry may necessarily be different from those used by the utilities. A primary example is the electrochemical industry, and particularly the chlor-alkali industry where salt brine is converted into chlorine and caustic soda, NaOH, by electrolysis. One of the advantages of fuel cells for this application is that they generate direct current. One of the things which is not readily appreciated is that a large fraction of the electrical power produced in the United States is consumed as direct current. Between the aluminum industry, the chlor-alkali industry, the plating industry and other metallurgical applications, about ~~[illegible]~~ 12-13% of all electricity in the U. S. is used as direct current. This means that about one in ~~[illegible]~~ eight kilowatt hours end up as direct current at the point of consumption. If you have a generating device which produces direct current, such as fuel cells, MHD, thermionics, etc., then these would be the preferred technologies for providing power for these applications. For the current and voltage levels at which the electricity is used it is not practical to transmit the direct current any appreciable distance, so it would have to be generated on-site, with plants normally running in the range of 20 to 100 MWe.

If I may get slightly technical for a minute, I have a flow chart which shows the potential use of phosphoric acid fuel cells for a 1000 ton per day plant producing chlorine and caustic soda. The data is given for a plant which uses conventional diaphragm conversion cells. As shown, a major portion of the electricity is consumed as direct current with about 20% being converted to alternating current to power various pumps and motors in the plant. One important energy input to the chlor-alkali plant is steam, which is used to treat the brine and to concentrate the caustic soda product for shipment. This steam can be supplied from waste heat generated in the fuel cell, a cogeneration aspect which further improves efficiency.

One important byproduct of the electrolysis of brine is the hydrogen which is produced. In normal chlor-alkali plants it is burned under a boiler to produce part of the steam required for product concentration, or simply flared. With the fuel cell power plant this hydrogen can be cleaned up, pressurized and used as fuel in the fuel cell. This may look like perpetual motion but it is not quite. The feedback hydrogen can provide only about 15 to 16% of the input fuel requirement, but that is an important saving.

At this point, we may note important differences between the 100 MWe fuel cell system required for powering a chlor-alkali plant, and the fuel cell systems which would be specified for the electric utility application. These are:

A. Most of the power is required as direct current, so that inverter equipment is required for only a small portion of the output,

B. There is a need for cogenerated steam, which is not provided by the electric utility fuel cell plant, and

C. Not all of the input fuel passes through the fuel processor.

Similar analysis could be performed, for example, for a refinery where there is a waste stream of hydrogen, or if molten carbonate fuel cells were available, for aluminum from bauxite production facilities where the cogeneration potential make such applications attractive.

The major point I would like to make is that these industrial applications, which for the most part involve cogeneration, can provide attractive rates of return to those industries which could and would use fuel cells if they were commercially available now. As with any of the emerging energy conversion technologies, the industrial cogeneration applications look like the most likely to succeed areas because of the leverage which they have over generation of electricity and thermal power in separate installations. While the electric utility market for fuel cells may ultimately be larger, I feel that the market for fuel cells to be used in industry is much nearer.

This is one reason I am pleased to be able to appear before this committee and comment on the Administration's proposal for fuel cell research and development activities in Fiscal year 1984.

With respect to the program which has been proposed, I do not concur with the concept that the present activities be drastically curtailed at this point. We are at the point where first generation technology is ready to be demonstrated in the field. While it may appear to be an expensive communication method, there is nothing like working hardware to get the message across to the decision makers in private industry. It is these decision makers, both manufacturer and user, who will ultimately decide the fate of the fuel cell industry in the United States.

I would respectfully recommend that the DOE program include some small level of effort to be directed at further definition of the industrial and cogeneration fuel cell application areas in order to more clearly define the equipment needs of the respective industry segments. I have found that investigative studies which are co-funded by both the government and industry develop a high level of awareness within the industry of both the advantages and disadvantages represented by a given technology. The real problems emerge and the potential benefits get quantified in a much different way than has happened in previous government efforts, such as the ECAS studies where the potential equipment suppliers were conducting the surveys. Having these studies conducted by or through the respective industry associations would bring a broad level of opinion to bear on the topic and would provide a ground for industry concensus on the applicability of the technology, and in what form. This would provide the equipment manufacturers with a clear guide for assessing the non-utility market and for fashioning products and systems for that market.

While our Industrial Fuel Cell Users Group will eventually develop the same criteria over the period of several years, I am concerned about the timeliness of the information in order to have an impact on U. S. manufacturers before foreign manufacturers have moved strongly into the market. For this reason I recommend that these activities be identified as a line item in the Department of Energy budget, and be afforded a modest level of support. I believe that as the program develops, and the benefits become clearer, that there will be much more incentive for industrial concerns or groups to participate in, and fund a larger share of the costs of demonstration of fuel cell systems and concepts. This will be because the benefits to industry will be achievable at a lower level of technology development than would be required for the non-industry applications.

An important part of any demonstration program is that there is hardware to demonstrate. To this end I support the continued development of advanced concepts such as the improved phosphoric acid fuel cell, the molten carbonate fuel cell, and the solid oxide electrolyte fuel cell. From an industrial users standpoint, development of the improved phosphoric acid fuel cell is the most important to the utility applications and they, through these research organizations, have a strong interest in seeing it developed. From an industrial users standpoint, many of our members would be just as happy if the present generation of phosphoric acid fuel cell stack assemblies were available at commercial prices.

Over the long run, the molten carbonate and solid oxide fuel cells will probably be most important to industrial users because of their higher efficiency, and the fact that the waste heat is available at higher temperatures, which makes it more valuable for process heat applications in many industries. These considerations are not as important for the utility applications. We therefore strongly recommend that technology development on these systems be continued until they are ready for the industrial marketplace. One of the planned activities of our Industrial Fuel Cell Users Group is to further define the benefits to industry which could accrue through industrial adoptation of these technologies. Because of the high risk nature of these two technologies, I do not see industrial users pursuing technology development in these areas, except where they are component suppliers, but only participating in demonstration activities after the hardware is developed.

Thus far the primary emphasis on fuel cell system development has been as a replacement of central station electrical power generation capacity. For these applications the source of electricity is transparent to the user who has no way of knowing whether the power came from a hydroelectric dam, a coal fired power plant, a nuclear power plant or a fuel cell, except when he gets his bill. The point I am trying to make is that for these applications, the fuel cell system is trying to compete with conventional power sources which have been fully developed to mature technologies over a hundred year period, with efficiencies honed to a fraction of a decimal point, capital costs minimized with multi-component variational analyses, and operating experience which projects 30 or 40 year economic life. For these reasons it is believed that the use of fuel cells in the industrial cogeneration mode is more likely to succeed in a commercial sense at any early level of development.

While some within the Administration may not agree, there is the view that it may be part of the Federal Government's role to provide support for front-end development efforts which will allow fuel cell systems an opportunity to compete with these mature systems in this market.

To justify Federal support for the development of fuel cells, an interesting predecence may be drawn with respect to gas turbines for electrical utility applications. Similar to fuel cells, gas turbines were first developed for governmental applications; the gas turbine to power jet fighters; and the fuel cells for power in space. The amounts of Federal funds spent on developing jet engines for the military programs were non-trivial. After gas turbines had reached a fairly high level of technical development, they were then presented in the commercial market to the electric utilities, as aircraft derivative engines, with essentially 99% of their development covered by military appropriations. There has been some military development of fuel cell systems for both propulsion and for quiet, portable power sources, but this level of activity is still about four orders of magnitude less than what is presently going into military gas turbine (jet engine) development. One important consideration may be that the technology base being developed by the Department of Energy under this program is directly applicable to Department of Defense missions. This may be cited as another reason for continuing Federal support of fuel cell system development at its present level of activity.

I see a parallel between development of the gas turbine and the phosphoric acid based fuel cell systems. In many ways the phosphoric acid fuel cell systems are similar to gas turbines in their potential roles within the electric utility industry. They both essentially offer low capital cost installations, but require a premium fuel, with the fuel cell system offering slightly higher efficiency than a gas turbine-combined cycle plant. I think that fuel cell systems will eventually play a similar role to gas turbines in the utilities overall generation resource plan, and as such will not be used as base load units. However for industrial applications, high load factors, or what would be called "base loading" is a general rule. Therefore the design of the fuel cells themselves for these applications may be different than the designs for utility applications, which is not usually the case with gas turbines.

I appreciate this opportunity to address the committee and express my views on fuel cells for industrial application. For those who are interested in more technical details, I have prepared several papers for technical society meetings which have addressed cogeneration using advanced energy conversion technologies and these can be made available to committee members and staff if so desired.

Are there any questions?

# INDUSTRIAL FUEL CELL
# USER'S GROUP

Group Members

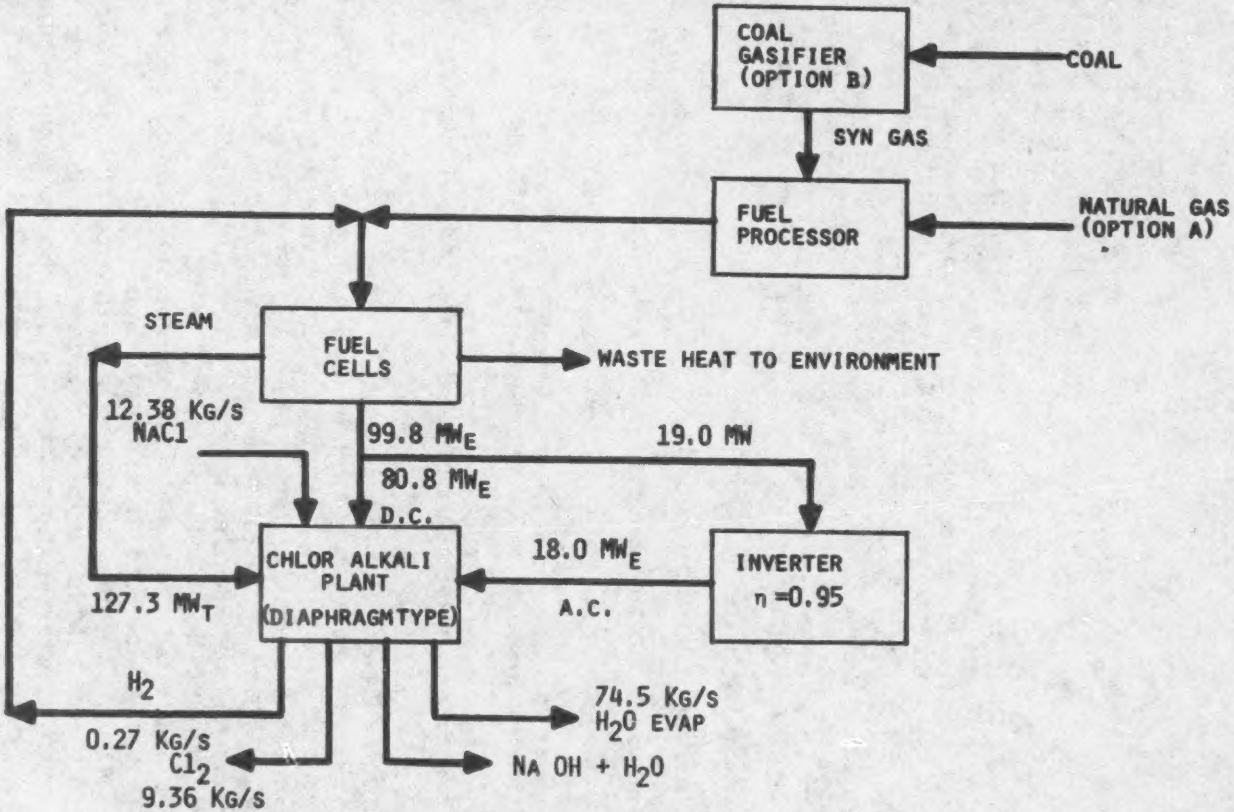
| | |
|---|---|
| A. D. Little | Brian M. Barnett |
| ARCO Petroleum | Julian M. Babad |
| Bergen County Utilities Authority | Herman Zablatzky |
| Chemetics | Ian H. Warren |
| Chevron | A. Robert Wasson |
| Chicago Sanitary District | Robert Barbolini |
| Dow Chemical company | Gordon Liu |
| Electech (Diamond Shamrock) | |
| Engelhard | Michael J. Brand |
| EBASCO | Malcolm Jones |
| Fluor | James Joiner |
| LCP Chemical | R. J. Burjett |
| Olin Chemical | J. L. Mays |
| Occidental | Al T. Emery |
| Penn-Walt | F. M. Shannahan |
| PPG Industries | Joe M. Anderson |

Strong Interest Indicated:

- Celanese
- Ethyl
- FMC
- Goodrich
- Shell
- Stauffer
- Vulcan Material

Endorsing Advisory Groups:

- DOE
- EPRI
- GRI
- IGT
- TVA


COAL
COAL GASIFIER (OPTION B)
SYN GAS
FUEL PROCESSOR
NATURAL GAS (OPTION A)
STEAM
FUEL CELLS
WASTE HEAT TO ENVIRONMENT
12.38 KG/S NACl
99.8 MW$_E$
19.0 MW
80.8 MW$_E$
D.C.
18.0 MW$_E$
INVERTER
η =0.95
CHLOR ALKALI PLANT (DIAPHRAGMTYPE)
127.3 MW$_T$
A.C.
H$_2$
0.27 KG/S
74.5 KG/S H$_2$O EVAP
Cl$_2$
9.36 KG/S
NA OH + H$_2$O

**USE OF FUEL CELLS IN CHLOR-ALKALI INDUSTRY**

March 17, 1982

BIOGRAPHY OF MALCOLM S. JONES, JR.

Mr. Malcolm S. Jones, Jr. is Manager of Advanced Technology & Special Projects for the Ebasco Services Incorporated Regional Office in Santa Ana, California. He is responsible for projects in the advanced technology areas such as: ocean thermal energy conversion (OTEC), solar thermal, wind farms, fuel cells, geothermal, municipal solid waste and magnetohydrodynamics (MHD).

He is a registered Professional Mechanical and Civil Engineer in the State of California and registered Sanitary Engineer in the State of Washington. He is a graduate of the Univeristy of Washington with a Bachelor's degree in Civil Engineering and he did graduate work in Physics at Rutgers University.

His professional affiliations include:

American Society of Mechanical Engineers
The Combustion Institute
Ocean Energy Council (OEC) (Member of Board of Directors)
Geothermal Resources Council
DOE Molten Carbonate Fuel Cell Industry Users Advisory Panel (1979-1980)
Captain, U. S. Naval Reserve (retired)
Chairman of the ASME Energetics Division, Subcommittee on MHD and EHD (1968)
Member of Steering Committee for Ninth, Tenth, Eleventh, Twelfth and Thirteenth Symposiums on Engineering Aspects of MHD
Member of the Federal Council for Science and Technology Energy R&D Goals, Study Panel on Advanced Cycle Fossil Powered Central Station Plants (1972)

Mr. FUQUA. Thank you, Mr. Jones.

Dr. Kolb, how near are coal liquefaction processes to reaching commercial maturity from the generic point of view?

Dr. KOLB. I am afraid I cannot personally answer that question, Mr. Fuqua. We do relatively little research in coal liquefaction at Aerodyne. I do believe earlier testimony from EPRI and other organizations would be more relevant on that point.

Mr. FUQUA. What are some of the primary areas in coal science that you think we need to stress attention on?

Dr. KOLB. In my view, the most important current area of research involves efforts to clearly understand and demonstrate direct coal combustion methods which are both economically sound, in terms of plant life and the ability to keep a plant operating with as little downtime as possible, and produces minimized environmental impacts, both near the plant, in the form of direct gaseous and particulate emissions, which may have adverse health and air quality effects, and also important long-distance effects, such as acid rain and general photochemical conversions over long distances.

Mr. FUQUA. Do you think, technologically, that we can achieve the 90-percent reduction in $SO_2$ that has been referred to, that would be the level we would have to get to to control acid rain? We are in probably the 50- or 60-percent areas that may be attainable and economically feasible at this time. Where should we be going in that area?

Dr. KOLB. There are quite a number of technologies which have been demonstrated in the laboratory scale and on a slightly larger pilot scale to achieve sulfur oxide removals of greater than 90 percent. So I do not think there is any question that, from a scientific point of view, such reductions are quite feasible.

What, in my opinion, is missing——

Mr. FUQUA. But that has not been demonstrated on a commercial scale.

Dr. KOLB. In general, these have not been demonstrated on a commercial scale, and, in my opinion, the reason for that is that we have not done the corollary research and development needed to scale these processes from the laboratory, or small pilot scale, to demonstration scale and, ultimately, to use in utility and industrial applications.

I think it is important to remember that a laboratory demonstration shows only scientific feasibility and full scale application often requires quite dramatic increases in materials capability and also involves nonlinear scaling of fluid mechanics, radiation transport, and mass transport affects, which are, in general, difficult to predict in terms of their impact on full scale operation.

There have been only a few technologies where the required effort to understand the scaling relationships and to understand the materials implications of having a process go from the laboratory to a utility scale application have been expended.

Mr. FUQUA. You mentioned in your testimony that MHD was a program in which there was probably a lot of money wasted, but to eliminate it, if I recall correctly, was not a prudent thing to do. What level do you think MHD should be funded?

Dr. KOLB. There are at least two levels which I think might be considered. The first is a relatively modest level of funding which would include data collection analysis from the existing demonstration facilities coupled with very selected areas of long-term research to get at those few very crucial materials and electrical plasma handling problems which we can see are the bottlenecks for commercialization of MHD technology.

I would guess—and it is only a guess—that a program of possible $10 million a year would be more than sufficient to keep our essential investment in MHD from withering as people leave the field and as facilities are destroyed or converted to other uses.

Mr. FUQUA. They are asking for around $40 million, as I recall.

Dr. KOLB. The second level of investment which is reasonable to consider is a continued operation of the existing demonstration plants, which indeed consumes considerably more funds than would be necessary to keep a purely research effort alive.

I think this option is worth considering, since a very substantial investment has been made, particularly in Montana for the CDIF facility. Whether or not that facility should be exploited for at least 1 or 2 more years in order to gather data on a demonstration project of that size is a reasonable question to ask. A tremendous investment has been made, and to date very little has come back. Thus it does make some sense to ask whether or not an increased level of effort should be dedicated beyond the research level to try to get some really useful data from that facility.

Mr. FUQUA. Dr. Leber, what does your crystal ball tell you about the commercialization of phosphoric acid fuel cells for central power generation?

Dr. LEBER. Mr. Rizk will address that.

Mr. FUQUA. Fine.

Mr. RIZK. I think the 4.8 megawatts in New York City will be starting later this year, and the commercialization should come within about 6 or 7 years from now.

Mr. FUQUA. At about the 4.8-megawatt level?

Mr. RIZK. No. It will be in the level of 11 megawatts done by United Technologies and 7.5 megawatts by Westinghouse. I think Westinghouse will probably be maybe more than 5 years from now.

Mr. FUQUA. And what will be the probable cost per kilowatt?

Mr. RIZK. I don't recall. Isn't it $3,000?

Dr. LEBER. $3,000 to $5,000 for the prototypes.

Mr. RIZK. $3,000 to $5,000.

Mr. FUQUA. $3,000 to $5,000?

Mr. RIZK. Yes.

Mr. FUQUA. Mr. Jones mentioned something in his testimony that was not clear. Maybe you could clear it up. You were saying you never thought—if I am paraphrasing correctly from your statement—that fuel cells would be used for base-load power. I assume that you mean that they would more likely have a better application in peak loads.

Mr. JONES. Yes, peak load and intermediate loads, but not in the base-load mode. Fuel cells require a clean fuel which will always be more expensive than coal for a baseloaded steam plant.

Mr. FUQUA. Right.

Maybe, Mr. Twitchell, you can answer this one, or Dr. Leber. Are there any incentives in the electric utility field, either publicly owned or privately owned, to encourage commercialization of fuel cells?

Mr. TWITCHELL. The incentives in the electric utility industry are, of course, the potential to utilize the technology to solve a number of problems. In the instance of rural areas, for example, very often there are loads far remote from major generating or transmission facilities that fuel cells could be utilized with automatic control to avoid the construction of large transmission facilities.

The funding for the fuel cell programs, the demonstration projects coming up, the electric utility industry will be spending approximately $300 million over the next few years on three to five demonstration plants. This type of demonstration plant is needed to bring the technology to the point that someone like our utility could utilize it.

But, as far as direct incentives, other than the hope of developing the technology, I cannot think of any direct money.

Mr. FUQUA. Thank you.

Yes, Mr. Jones?

Mr. JONES. The utilities work under the concept that they plan their generation in such a way that they lower the required revenues which they have to receive through their rate structure.

Studies have been made which suggest that if you put the fuel cell systems in a proper portion of the power generation spectrum, fuel cells can be competitive, at a cost which is in the range of $700, $800, maybe $1,000 a kilowatt, With their higher efficiency, and a capital cost of this magnitude, the studies project that, if the fuel cells were available, they would result in a lower required revenue for almost any utility system.

That is the context, I think, in which the utilities will eventually end up using their fuel cell systems. They have a lot technical of advantages, but lower required revenue is the one that will drive their widespread application.

Mr. **Fuqua**. Thank you, sir.

Mr. Bateman.

Mr. **Bateman**. Thank you, Mr. Chairman.

Dr. Kolb, in your statement—and I am not critical of it, because those of us on the committee and the witnesses, by and large, who appear before us have our particular points of emphasis and goals that we feel are eminently appropriate to be pursued with a great deal of vigor—you indicate that the only justification for budget reductions of the magnitude that have been suggested is that we either feel we have solved the problem or that we overestimated the dimensions of the problem or that someone other than government has been found who is going to solve the problem for us.

Doesn't that leave out one other criterion that enters into the rationale of some for reduced appropriations, namely that we just flat out don't have any money?

Dr. **Kolb**. Well, I am, of course, very sensitive to the fact that we need to spend our Federal revenues in as wise a way as possible and that they are very limited.

However, I would submit that the problem will cost us a great deal more to solve if we abandon the level of effort which has been funded since the Department of Energy has come into existence and pretend that the problem is not there for some period of time.

In my experience, problems which are ignored and deferred get bigger and uglier very quickly, and I believe that this problem is one of that type. So I think that even though investment is painful, if we don't make that investment, we will end up paying later rather than paying now, and paying more.

Mr. **Bateman**. I remember that commercial, "You can pay me now or you can pay me later."

Well, certainly I have no disagreement with that perspective, and it would be my hope that I can find a way to support some enhancements of funding in these programs.

But isn't there something also to be said that, given the public moneys expended over the last several years and the demonstration of a scientific or technological capability to do things, Government can and indeed should be expected to play a lesser role, the technology base having been developed, and let the marketplace play its role through the private sector in the broader utilization of these technologies as the economics of our energy problem manifest themselves?

I guess I am saying, in a very long way, there are lots of technologies available that look extremely good and would advance at a very accelerated pace if the cost of oil were $50 a barrel, that just are not there if it is $29 a barrel, and that you do have to accommodate to those economic realities.

Perhaps, to put it in gross and exaggerated terms, we cannot spend millions of dollars demonstrating today that we can produce our energy needs by some alternative methods when we do not need to incur that cost to solve the immediate, today's, energy problems.

Dr. **Kolb**. I certainly have no quarrel with the philosophical principle that you have expressed. I believe it is absolutely correct. What I would like to suggest is that we need to look very carefully at what level we can cut back to.

The reason that I feel that that careful look is necessary is that technology is, in some sense, a living thing. While it makes a great deal of sense not to pursue too far into the demonstration and implementation stage technologies which are now not economically competitive and do not have any great hope of being economically competitive, I would submit to you that if we abandon all our research and development efforts, we will lose an entire generation of scientists and engineers who have, with a great deal of effort and investment, come to grips with the problem.

I think that it is quite important that a baseline level of activity in this area be maintained so that we retain the intellectual and, to some extent, physical resources to deal with these problems. To fail to do that is essentially eating your seed corn. When the famine comes again, you are in very dire straits indeed.

Mr. **Bateman**. I certainly would also concur in that philosophical approach.

Maybe now I should get a little bit more specific. Let us assume that, just to bring a figure from the air, the committee in its wisdom were to recommend a funding level that included an additional, say, $100 million for fossil fuel research. Is it your view that that money, if augmenting what was already proposed, could and would be wisely spent? Is there a priority list as to, if you have $100 million more, where you should spend it and who should get it for what projects and where those projects would be undertaken?

Dr. **Kolb**. I believe that the Department of Energy fossil research and development staff, that is the professional staff of the Department of Energy, is quite capable, even in its reduced numbers, of planning and administering a cost-effective program at the level of, for instance, the fiscal year 1983 budget, which would be about what you have suggested.

In terms of the absolute wisdom of all of the decisions made by DOE staff, I think all of us have some differences with their program plan.

Mr. **Bateman**. Sure.

Dr. **Kolb**. I would like to point out that at that level of budget, it becomes very, very important that we pick the expensive demonstration projects, as a component of that budget, very wisely, because, at the fiscal year 1983 funding level, only a very few demonstration projects can be financed, even with the cooperation of EPRI and with industry contributions.

The major worry I would have at the budget level you have suggested is that there is always a tendency to support demonstration projects, which bring jobs and bring high visibility, at the expense of maintaining the basic research and engineering capability, where the jobs are much more dispersed and the individual accomplishments are much less prone to be reported in the evening newspapers.

**Mr. BATEMAN.** Mr. Chairman, if I have a minute or so further, I have been here at the committee table, and I don't know how many times today I have heard members of the panel refer to phosphate acid fuel cells——

**Mr. JONES.** Phosphoric.

**Mr. BATEMAN.** Phosphoric acid fuel cells. See, I can't even get it right, as often as I have heard it. I am probably the only person in this room who does not know what you are talking about, but since I do not, would you please describe conceptually what these things are?

**Mr. RIZK.** The fuel cell essentially takes hydrocarbon fuel and converts it to hydrogen. The oxygen comes from the air; the hydrogen comes from the hydrocarbon. When they are combined together, they generate electricity like a battery.

So the phosphoric acid is just an electrolyte in that mechanism.

**Mr. BATEMAN.** The hydrocarbon you speak of I assume would be coal?

**Mr. RIZK.** It is any hydrocarbon, like the naphtha, like benzene, like methanol, anything that contains hydrogen atoms.

**Mr. BATEMAN.** Oh, all right.

**Mr. RIZK.** So the whole trick here is that there is a catalytic conversion.

**Mr. BATEMAN.** Do I now have one? [As he is handed a model of a fuel cell.] [Laughter.]

**Dr. KOLB.** A small version.

**Mr. BATEMAN.** Well, I thank you, and that is helpful. I am beginning at least to grapple conceptually with what this phenomenon is, and I will pursue some more independent research on it.

**Mr. RIZK.** The good thing about the fuel cell is the effluent that comes out of it is pure water. You have hydrogen and oxygen, and there is no pollution whatsoever. There is no noise pollution because there is not much mechanical work going on, like turbines and so on. Essentially, it converts liquid fuel to electricity more efficiently than burning that fuel. That is the underlying value of it.

**Mr. BATEMAN.** It sounds as if we need one of those very badly in southeastern Virginia. [Laughter.]

**Mr. JONES.** Could I enlighten Mr. Bateman a little bit? The fuel cell is like a battery which you feed chemicals into. The phosphoric acid is very similar to butter in a sandwich. You have two electrodes which are represented by the slices of bread, one of which carries the electricity out and the other carries it back in, and the phosphoric acid which is between two plates and provides the continuity of electrical current between the two electrodes.

It is the material in between, which is the electrolyte. The type of electrolyte is often used to differentiate between the types of fuel cells. In the molten carbonate cell, you have a different material, lithium potassium carbonate which operates at a higher temperature to carry the current within the cell. In the solid oxide electrolyte fuel cell you have a different material, usually a doped mixed oxide, which is a ceramic, and which must be at a higher temperature to conduct electricity. That is where they basically differ.

But it is between the plus and minus electrodes where the material is that we are referring to.

Mr. Bateman. I thank the panel and the Chair for its indulgence in the further education of Congressman Bateman.

Mr. Fuqua. Mrs. Schneider?

Mrs. Schneider. Mr. Jones, I find your industrial fuel cell users group to be very interesting insofar as developing the utilization of fuel cells among American industry. I wonder to what degree you have had conversations or communications with DOE.

Mr. Jones. We had DOE representatives at our initial organization meeting in Newport Beach, and members have had some discussions with the DOE fuel cell program management office at Morgantown, W. Va.

Mrs. Schneider. Do you plan on approaching them for some similar type of activity?

Mr. Jones. Like the electric utility fuel cell users group, that kind of coordination.

Mrs. Schneider. I see. Have you had any communication with other kinds of groups similar to your own along the lines of coalition building?

Mr. Jones. I am a member of the Ocean Energy Council, and so I am a little bit familiar with coalition building.

Mrs. Schneider. I see. So that is the only other group.

One of you mentioned in your testimony that through 1982 the private sector had provided about $184 million for the fuel cell program. I wonder if you can provide us with any details to substantiate that.

Dr. Leber. These funds are derived primarily from the companies, the prospective manufacturers of the fuel cells, from the edectric utility industry, bgth as individual utilities and through the Electric Power Research Institute, and through smaller contributions that derive from fuel cell component manufacturers.

Mrs. Schneider. But this is an actual figure? This is not just a geferal assumed figure?

Dr. Leber. No. We have, of course, been very aware of the feed to carry our share of the load, and for the past several years we have been very carefully tracking the private sector investment and comparing that to what has been derived from government sources. As we have reported in this morning's testimony, that ratio remains on the order of $2 of private funds to every dollar that has been invested from the Federal sources, and we do expect that to at least remain constant and probably grow in the near future.

Mrs. Schneider. Hopefully grow, seeing as how the sentiment you have gotten from this committee today certainly, I would think, sends a clear signal that we are trying to keep inflation under control, bring interest rates down, and all those other noble things, and obviously we have to do some trimming here and there.

So if, as you had said, Dr. Kolb, that the philosophical agreement earlier with Mr. Bateman is that you do feel that the private sector should proceed, obviously it is going to have to proceed by putting more money where its mouth is. That would allow us to do all the other good things that we are supposed to be doing here.

Dr. Leber, I wonder why you favor programs other than basic research for primary funds and basic research for secondary or leftover funds? Could you elaborate a little bit on that, please?

Dr. **Leber**. Well, I think some of the other witnesses here indicated how important it is, what a strong statement it is, to actually see the device out in the field, to be able to "kick the tires." This is visible testimony to the technical viability of a particular device or technology.

Our focus at this point is, first and foremost, to get a fuel cell out in the field and get it operating to prove to the interested parties that it does work and it is more than a nice theory or a good concept. This is our focus for the next 2 or 3 years. We are at that delicate stage of moving forward into some commercial prototype demonstrations and, hopefully, by the end of the decade, into full commercialization.

Rather than adding a lot of bells and whistles to the technology at this juncture, we feel that we have to move forward with the closest technology that we have available, which is the phosphoric acid technology, and to get it out into the field.

At the same time, we recognize, as Mr. Jones has pointed out, that down the road 10 years and possibly 20 years, there are conceptual technologies that will have higher efficiency, that will have better utilization of the fuel, that will provide temperatures for cogeneration that are somewhat more attractive. We do believe that, in the longer term, those technologies deserve attention. But we want to retain this near-term focus to prove the concept and to bring the private sector and the marketplace into line so as to move forward with commercialization in 1990 and following.

Mrs. **Schneider**. All right, good enough. Thank you.

Mr. **Bateman**. If I may, I would like to get back into this new dimension of fuel cells. This is a technology which you feel will have matured by the early 1990's. Do you predict that that is going to be something that emerges first in the context of direct power user capability or facilities, or is it something that will be dominated initially by the utilities who generate and transmit power, or will both develop more or less abreast of one another? What should we look for there?

Dr. **Leber**. Maybe I could take a quick shot at that. I think that we have gotten the flavor of diverse interests at this morning's hearing. Clearly, there is a very strong and growing interest on the part of the industrial users to couple the fuel cell into their process, both as a chemical component as well as a thermal and electrical component.

There is growing interest on the part of many of the gas utilities and their customers to investigate smaller scale fuel cells, in the range of a few tens of kilowatts. At the same time, the electric utility industry is examining larger, multimegawatt units, both as stand-alone electric generators and cogenerators.

It is very apparent that this is a technology that has appealed to a wide range of users, I am not sure which one is going to be first. We do have investigations going on across the country, detailed studies that examine everything from the cogeneration applications, based perhaps on coal or some other fuel, to the smaller scale applications in laundromats and telephone switching stations.

So there is an element of parallel progress here. Which one actually gets to first base first, I am not honestly certain. But I can attest to the growing level of interest across the board.

Mr. **Bateman**. Yesterday, for me, the new phenomenon was this thing called MDH, which has been referred to today. Excuse me, that is MHD. See, I have a low level of recollection, but I am glad we are on the same wavelength.

Yesterday's testimony included information to the effect that the Energy Research Advisory Board had recently published a report which was substantially, if not entirely, negative in terms of MHD. Did I say it right that time?

The testimony today suggests that the program should be supported, but perhaps not as high a level as some are urging. If you take the potential of that technology and compare or contrast it to fuel cells, which has the greater potential? Which deserves the greater emphasis?

I am sure, from what I have heard, you believe that both are worthy of continued federally supported research and development. But can you tell us where the emphasis perhaps should lie, which has the greater potential?

Mr. **Jones**. I would like to address that since I have worked in both fields.

I think the MHD is more practical in the larger size, the 1,000-megawatt, to 500-megawatt size plants for central station applications, as a way of burning coal cleanly and with a reasonably high level of efficiency.

Those kinds of applications require, essentially, billions of dollars to get the technology up to the level where it is commercial, and I am not sure that the electric utility industry is ready at the present time to make that kind of investment. So I view MHD as coming along at about the year 2000, where it would be at a technology level if we keep doing the things we are doing, that it could start to come into the commercial field.

We presently have the 4.8-megawatt fuel cell system in New York and will soon be on line as an operating system, so that in the shorter time frame, I can see the fuel cells as becoming a very important part of the utility system.

But the fuel cells, as contrasted to the steam powerplants, become very efficient at small sizes. The 10-kilowatt or 100-kilowatt fuel cell is about as efficient as it is ever going to get. So they will tend to be used for applications such as dispersed generation which do not require the thousands of megawatts of power which are inherent in central station power generation.

So they fit two different parts of the electric utility generation requirement. They will come into being in two different time frames.

Mr. **Bateman**. I have exhausted any questions that I have for the panel. The chairman, having had to go elsewhere, asked if I would close the meeting.

Before closing it, if any of you gentlemen have other comments that you would like to get into the record, I may be beyond your capacity to educate, but you will at least be able to get things into the record if you have further comments you would like to make.

Mr. **Jones**. I would like to make one comment with regard to the role of private industry in bringing about demonstration plants.

One of the problems, I think, is that we have too many lawyers and accountants to run companies and not enough engineers. [Laughter.]

So it makes it very difficult for innovative people to carry out very large demonstration projects within the private sector. I was fortunate to be able to work for the late Mr. W. G. "Billy" Reynolds at Reynolds Metals Co. He was one of the people who had vision about these kinds of projects, but he was alone, essentially, in industry for having that kind of view.

The present ways we run corporations, our present laws relating to capital formation, just make it very highly unlikely that those things will come about in the near future.

Mr. **Bateman**. Well, that does suggest a further question.

We have focused today in terms of Federal direct appropriations in aid of research and development in these areas. Are there things that government can do which do not require direct appropriations from the Treasury, refinements in or new tax incentives, deductions, or things which make the private industry's capital go further?

Mr. **Jones**. In my other capacities, I advise people about how to use the investment tax credits and other things, and those are just another way of making the Federal Treasury pay for the things. You do not have to appropriate the money directly; you stop it before it comes in.

Mr. **Bateman**. It seems to be a little more painless a process. [Laughter.]

Mr. **Jones**. But when I fill out my 1040 form, I am still paying for it.

But I think it goes further than that in the terms of a basic reform of the laws that relate to capital formation.

Mr. **Bateman**. There are some that believe that those relate directly to reducing the level of Federal expenditures and size of deficits, so we almost meet ourselves coming back, which presents the classic dilemma, I guess.

Are there any other comments from anyone on the panel?

Dr. **Leber**. I would just like to thank the committee for its longstanding support for the fuel cell program. This committee has been very consistent and provided outstanding guidance to the industry and to the Department of Energy in their conduct of the fuel cell effort.

I have just learned that the Japanese have tripled their budget for phosphoric acid fuel cells and that, more than ever, we require the same kind of consistency and support that this committee has provided, because we are in an international competition here for this technology.

Mr. **Bateman**. I am quite sure that my colleagues who have been on the committee and who are not, unlike me, new to it will especially appreciate those remarks being in the record.

If there is nothing further, then, I suggest that we adjourn the meeting.

[Whereupon, at 11:50 a.m., the subcommittee recessed, to reconvene at the call of the Chair.]

[Additional testimony submitted for the record from Bernard S. Baker, president, Energy Research Corporation, follows:]

## Statement of Bernard S. Baker
President
Energy Research Corporation

My name is Bernard S. Baker. I am President of Energy Research Corporation, a subsidiary of St. Joe Minerals Corporation, a Fluor Company. I am a Chemical Engineer and have been continuously active in the fuel cell field since 1959.

Energy Research Corporation, or "ERC", is a small company with approximately 130 employees, located in Danbury, Connecticut. ERC is a leader in the research and development of energy-efficient power conversion and storage devices, and is the only American company to manufacture and sell both phosphoric acid and molten carbonate fuel cells and advanced batteries. Its team of technical specialists is among the largest electrochemical groups in the United States.

ERC has had successful fuel cell development contracts with the Departments of Defense and Energy, National Aeronautics and Space Administration, Electric Power Research Institute and the Gas Research Institute.

Today I would like to briefly describe the fuel cell concept, then discuss a special type of fuel cell that is of considerable importance at this time.

### I. The Fuel Cell in Brief

The fuel cell is the most efficient electric energy conversion device in existence. Similar to a continuously fueled battery, the fuel cell produces DC electricity at high efficiency by silent galvanic reactions in a device containing few moving parts. Its only by-products are heat, water and carbon dioxide. The fuel cell is therefore quiet, safe and nonpolluting. Fuel cells generally are powered by hydrogen that can be derived from a variety of fuels -- including coal, biomass and natural gas.

Fuel cells can be built to dimensions ranging from kilowatts to megawatts and can be factory assembled. Their efficiency is relatively independent of size. As the efficient use of our nation's resources and replacement of oil by other fuels is critical to the American economy, increased energy efficiency by widespread use of fuel cells is one of the least expensive ways to help the U.S. achieve energy independence and to further reduce oil imports.

ERC is grateful for the recognition that this Committee has given to the important role that fuel cell development can play in our nation's energy future. This Committee has also recognized that such development needs and deserves the support and backing of the Government. We appreciate the support that this Committee has given to fuel cell programs and sincerely hope that its continued support will be reflected in its FY 1984 Budget Recommendation.

## II. The Direct Fuel Cell

Although in an early stage of development, the molten carbonate fuel cell appears to be the most efficient type of fuel cell. In addition, the molten carbonate cell produces a higher temperature waste heat that can be easily utilized for both commercial and industrial purposes. This type of fuel cell is constructed from low-cost materials abundant in the Western Hemisphere.

In the past three years, ERC has developed a new and highly efficient form of molten carbonate cell that we call the "Direct Fuel Cell." We gave it the name "Direct Fuel Cell" because the fuel cell stack consumes fuels like natural gas and coal-derived methanol directly -- without the need for externally-produced hydrogen as fuel. As a result, this system does not use the hydrogen generator, platinum catalyst, and extra cooling equipment required by other fuel cells, and therefore is 50% more efficient (See Figure 1) than other systems and has a much lower investment cost (See Figure 2).

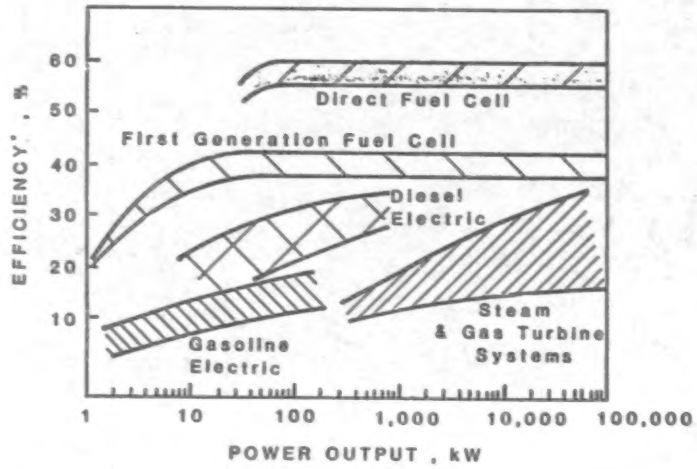
Because the Direct Fuel Cell is based on using modified molten carbonate-type fuel cell stacks, the waste heat produced by the process will be of a high quality and have substantial economic value. I believe the Direct Fuel Cell represents a major advance in fuel cell technology. It can be used as a small on-site integrated energy system. For example, a Direct Fuel Cell in the 100 kW size range would have a heat rate of 6000 Btu/kWhr and would produce about 2000 Btu of usable waste heat for every kWhr of electricity generated. This would result in an energy savings to the average customer in the Northeast of about $300-500 per year and a considerable reduction in oil imports to the region as a whole. The Direct Fuel Cell would be an attractive source of low-cost, environmentally acceptable power for multi-megawatt powerplants used as part of the electric utility grid system. Finally, in the long term, large coal-fueled systems could be built using this country's most abundant fuel.

We at ERC have discussed our Direct Fuel Cell concept with technical experts at the Electric Power Research Institute and Argonne National Labs. I believe they would support the statements I have made here today. Independent studies by these organizations verify our technical case.

Energy Research Corporation asks this Subcommittee to give specific recognition to the advantages of supporting this Direct Fuel Cell Program as a natural outgrowth of the Fossil Energy Molten Carbonate Program. The Direct Fuel Cell program requires several years of Government support to nurture and develop this concept. We urge you to enable us to continue our efforts in this promising area.

Thank you.

FIGURE 1



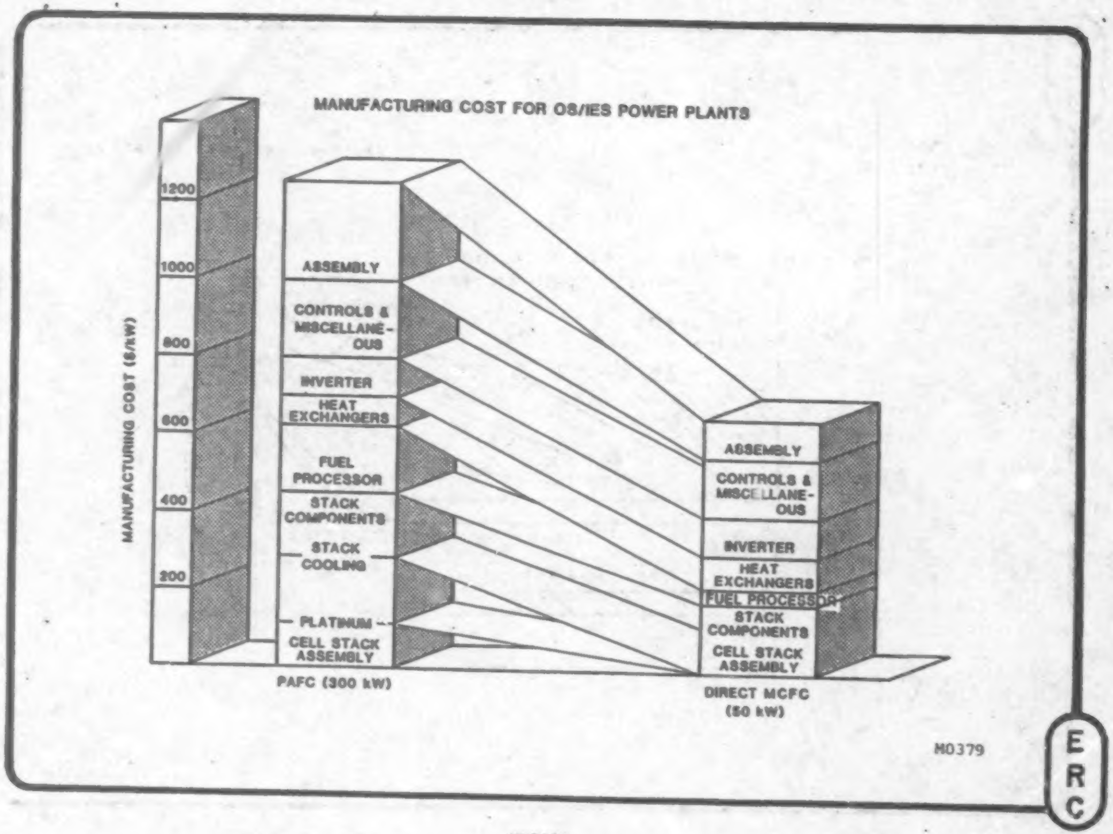
*Based On Lower Heating Value

D1036Br1

ERC

VM2070R

FIGURE 2



VM2084

J. Richard Lucas, Head
and
R. H. Yoon, Associate Professor
Department of Mining and Minerals Engineering
Virginia Polytechnic Institute and State University

MR. CHAIRMAN AND MEMBERS OF THE SUBCOMMITTEE ON ENERGY DEVELOPMENT AND APPLICATION

I am J. Richard Lucas, Head and my associate is R. H. Yoon, Associate Professor, Department of Mining and Minerals Engineering, Virginia Polytechnic Institute and State University. It is my pleasure to represent the University, Dr. William E. Lavery, President, which is the land-grant University of Virginia and the center of mining and mineral engineering research, instruction and extension in the Commonwealth of Virginia.

I and Dr. Yoon are grateful to you and the committee for extending an opportunity to offer this statement on behalf of Coal Preparation, Coal Research and Development, Fossil Energy, Department of Energy (DOE).

From a constructively-funded effort in coal research and development a few years ago, budgetary support has steadily been reduced and decreased to practically a level of insignificance considering the comparative and substantial potential that coal represents in meeting the future energy needs of this nation. Because of its substantial present as well as future promise, coal research and development should be more fully supported as an on-going program to permit increased effort in coal preparation to increase coal utilization in a number of growing consumer markets. Consideration should be given to increasing the funding of research in coal preparation research to improve its quality and quantity factors as a low-cost, economically-viable, indigenous source of energy. An increased coal research effort may do much to ameliorate presently this highly-depressed industry as part of our overall recessionary economy. The unemployment rate in the coalfields is more than double the average unemployment rate of this nation. It is our desire to speak to legislation, which may assist and expedite the return to work of coal miners within the short term and to improve the potential in the longer term of expanding the consumption of coal and increasingly displacing international oil as an energy source by increasing the amount of indigenous American coal prepared and processed more effectively by improved coal technology.

American coal is the lowest cost fuel source in the nation, which is available at approximately 1/4 of the price of oil and perhaps 1/2 of the price of natural gas. Electric utilities rely upon coal for more than 1/2 of their fuel needs, and this reliance has steadily been increasing in the past decade during the 1970's. With careful coal preparation employing new technologies developed by research and development efforts, the future of coal will be strongly enhanced in its consumption in an environmentally-acceptable manner. This is most important since the American electric-utility industry is no longer looking to the construction of new utility facilities fueled with feed stocks of oil and gas.

The potential of coal in displacing increasing amounts of oil and gas in the domestic market is readily apparent, but it also should be recognized that coal is displacing increasing amounts of petroleum as an international energy commodity especially in the electric-utility industry of the world. The competitive position of the environmentally-acceptable quality of American coal will be improved and steadily advanced by coal research and development beginning with improved technology in coal preparation.

An example of the need and an application of this new technology for coal has been strongly manifest within the past few years. A surge of interest has been initiated in developing methods of preparing "super clean" coal containing low percentages of ash, less than 2%, and low percentages of sulfur, 0.3%. This high quality product is particularly desirable in the production of coal-water mixtures (CWM), which contains roughly 70% solids and is stabilized by the addition of various chemical additives to permit pumping, storage and utilization as in the utilization of oil. Leading authorities including the Department of Energy have estimated that approximately 4.8 million bbl/day of oil, about 25% of the total normal daily consumption in the nation, may potentially be saved if oil-burning boilers are converted to coal. The difference between the price of coal, $1.50/million Btu, and oil, $6.00/million Btu, would easily accommodate the additional cost of preparing super-clean coal for coal-water mixtures.

At present, many coal and petroleum companies are actively engaged in developing processes for producing super-clean coal. Some claim that the ash content may be reduced to well below 1%, perhaps 0.3% or 0.4% ash, with over 95% recovery of the coal. Apparently, the washability of a coal depends upon its petrographic characteristics when physical coal-cleaning techniques are employed, and some coals are easier to clean than other coals. For this reason, it is essential to determine the characteristics of washability of the various coals mined in the United States for evaluation of the potential of the processes for obtaining super-clean coal. It is our understanding that the Department of Energy is currently undertaking this important task with the objective of disseminating this critical data to the industry.

It should be noted that only a few coal seams, mined in the U. S. today, have the desirable washability characteristics for this emerging purpose. In fact, most of the coal seams are very difficult to super-clean using conventional techniques of coal preparation. In order to develop the processes that may be used for a wide variety of coals, faculty researchers, Department of Mining and Minerals Engineering, VPI & SU, have developed a strategy for pulverizing coal to liberate the coal particles from ash-forming minerals and then processing the pulverized coal using a technique of froth flotation. A problem with this approach is that the fine coal particles are not readily amenable to the conventional process of froth flotation. Therefore, our researchers have developed a process using small air bubbles for the flotation of coal particles of micron size. Very encouraging results have been obtained with a variety of coal samples, but much additional research is needed to achieve a desired level of understanding and technology development.

Other chemical and physical techniques of coal cleaning exist and are potentially useful in producing super-clean coal. In general, the physical techniques of coal cleaning are intrinsically more economical than chemical techniques. Yet, the chemical techniques of coal cleaning should also be viewed as an economically-viable approach in light of the substantial economic margin between the consumption of coal and oil.

Super-clean coal may be an excellent feedstock for the newly-energing fuels such as coal-water mixtures and coal-oil mixtures. It may potentially replace approximately two-thirds of the 35,000 megawatts of electrical energy currently generated by boilers using oil as a feed stock. New electric-utility plants may also seek high quality premium coals for high combustion efficiency and the minimization of operating, environmental and transportation costs. Considering that No. 6 heavy oil, used in electric-power generation, contains up to 0.5% ash, the potential for consuming super-clean coal as a "clean fuel" is enormous.

As a result of the substantial potential for the increased utilization of coal both domestically and internationally in displacing increasing amounts of oil as an energy commodity, we respectfully urge that the funding for coal research and development in coal preparation for the fiscal year 1984 be restored to the level that existed a few years ago to support the important and promising research and development efforts in coal, which were, even then, financed by modest federal outlays.

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